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(*May*, 1851.)

HISTORICAL SKETCH

OF THE

INSTITUTIONS FOR THE INSANE

IN THE UNITED STATES OF AMERICA.

By Pliny Earle, M. D.

READ JULY 7, 1847.

About the middle of the eighteenth century, some philanthropists of Philadelphia took the preliminary measures for establishing a curative institution in that city; and in 1751, the Provincial Assembly of Pennsylvania passed an Act of incorporation, under the title—"The Contributors of the Pennsylvania Hospital." This charter provided, not for the relief of the sick alone, but also for the "reception and cure of lunatics." This particular provision for the insane by Legislative authority, together with appeals to the public and the official proceedings that were necessary to establish the institution, are unequivocal indications that, even at that early period, the true interests of the insane began to be properly regarded by the community. In Pennsylvania then, as in other states since that period, the wretched condition of those who were suffering under mental alienation, and the absence of all suitable receptacles where they might be comfortably accommodated and properly treated, were fully set before the people.

The Pennsylvania Hospital was opened on the 11th of February, 1752, and thenceforward one of its departments was exclusively devoted to patients suffering under disorders of the mind. For many years, it is said to have been superior to any other receptacle for the insane in this country.

Subsequently to the establishment of the Pennsylvania Hospital, for a period of nearly twenty years, no further

provision for the insane was made by the government of either of the American Colonies.

Virginia was the next state, so far as we have been able to learn, which took any active measures in the benevolent enterprise ; and to her belongs the credit of being the pioneer of all the colonies in first establishing an institution *exclusively* devoted to the insane.

An Act providing for the lunatics and idiots of the colony passed her Legislature on the 10th of November, 1769. During the following year, proposals were received for the construction of a suitable edifice, which was soon begun, and completed, at an expense of £1070, on the 14th day of Sept. 1773. The institution commenced operations, and continued until some period during the war with the mother country, when the building was vacated, and used for a while as barracks by one of the revolutionary armies. It was subsequently re-opened for the insane, to whom it has since been constantly devoted. Within the last few years it has, at two different periods, been greatly enlarged ; and still further additions are contemplated. It is known as the Eastern Lunatic Asylum of Virginia. The cost of the establishment, to the present time, has been about $73,000, and was defrayed entirely by appropriations by the Legislature of the state. It has a farm of twenty acres, and accommodations for one hundred and seventy patients. Its present number is one hundred and sixty-two.

Previously to the appointment of Dr. Galt, the present medical superintendent, the physician did not reside upon the premises, and had no further control over the patients than merely as a prescriber for the sick.

In the city of New York, a number of benevolent individuals having made subscriptions towards a fund for the establishment of a hospital, three eminent physicians, Peter Middleton, John Jones, and Samuel Bard, in the year 1770, presented a petition for a charter of incorporation to the acting commander-in-chief of the province. On the 13th of June, 1771, the Earl of Dunmore, then governor and commander-in-chief, granted the petition ; and a certain number of people were incorporated under the title, " The Society of the Hospital in

the City of New York, in America." The State Legislature, on the 9th of March, 1810, changed this title to the "Society of the New York Hospital." Soon after the original Act of Incorporation, the fund already obtained was increased by contributions from Great Britain, obtained through the influence of Dr. John Fothergill and Sir William Duncan, as well as by an annuity from the provincial Legislature of New York. A building was commenced, but before its completion, it was nearly consumed by fire, on the 28th of Feb. 1775. Being partially repaired, it was occupied during the revolutionary war as barracks by the British and Hessian soldiers, and occasionally as a hospital. On the 3d of Jan., 1791, the house was first in a suitable condition to receive patients. On that day the number of admissions was eighteen. The earliest period at which insane patients are known to have been received was in May, 1797. During that and the succeeding month nine cases of mania were admitted; four cured, two relieved, and one died.

From May to December, inclusive, 1798, twelve insane patients were admitted; and the average number resident in the institution, during that period, was seven. From the 31st Jan. 1795, to the 31st Dec. 1803, two hundred and fifteen lunatics received the benefits of the hospital. This class of patients having greatly increased, a separate edifice, called the "Lunatic Asylum," was erected, and opened for their reception on the 15th of July, 1808. Nineteen patients were removed to it from the hospital, and forty-eight others were admitted, making a total of sixty-seven. For thirteen years this building was exclusively devoted to patients affected with mental disorders.

In 1797, Jeremiah Yellot, of Baltimore, gave seven acres of land to the State of Maryland, on condition that the Legislature should establish a hospital for the treatment of insanity and general diseases. In the following year an appropriation for this purpose was made, which, being increased by subscriptions from individuals, and by the government of the city of Baltimore, was devoted to the construction of a suitable building. This was not opened, however, if we are correctly informed, until the year 1816. Since the 1st of Jan. 1838, it

has been devoted exclusively to the insane. The buildings have been enlarged since they were first erected, and the whole cost of the establishment has been upwards of $200,000. It can accommodate over one hundred and twenty patients; the present number is one hundred and ten.

Pursuing the chronological order which has been adopted, we must again return to Pennsylvania. In the year 1812, some members of the Society of Friends, desirous of improving the condition of the insane belonging to their religious denomination, associated themselves, and were subsequently incorporated as "The Contributors to the Asylum for the use of Persons deprived of the use of their Reason." Contributions having been obtained, a building was commenced about the year 1815; and completed and opened in May, 1817. It had accommodations for forty patients. About the year 1830 it was enlarged, so as to be able to receive sixty-five. This Asylum is situated in the township of Oxford, one mile west of the Village of Frankford, and about six miles north of the city of Philadelphia. It has a farm of sixty-five acres. The superintendent is not, and has never been a physician. The medical care of the patients is confided to an attending physician, who visits the patients twice a week, and an associate who resides at the asylum.

By an act of the Legislature of Massachusetts, passed in 1810, an institution to be located in the city of Boston, was incorporated under the title of "The Massachusetts General Hospital." In subsequent years, a fund of fifty thousand dollars was raised by subscription, for the purpose of establishing a department for the insane. A building was commenced in 1816, completed and opened for the reception of patients, on the 6th of Oct. 1818. It is about two miles from Boston, between Charlestown and Cambridge, in a district which has recently been incorporated as the township of Somerville.

At the close of the year 1820, the number of patients was twenty-three. In 1821, John McLean, of Boston, bequeathed the sum of $100,000 to this institution, and since that time, in accordance with a vote of the corporation of the hospital, it has been known as the "McLean Asylum for the Insane."

With the fund thus obtained, two new buildings were erected for male patients. In 1835, Miss Mary Belknap, of Boston, left to the asylum a legacy of nearly $100,000, with which the female department was enlarged by the construction of buildings suitable to those just mentioned.

The heirs of Francis Lee also gave the institution $20,000, then invested in the stock of the Boston and Worcester turnpike road. As the donation was coupled with the condition that this investment should be perpetual, the whole amount was lost when the construction of a railroad rendered the turnpike valueless.

Besides these liberal direct donations to this asylum, the trustees of the hospital, of which it is a branch, have received various munificent bequests, for general purposes. Of these the following are the principal:

From	Mr. John Redman, . . .	$100,000
"	Mr. Oliver, of Dorchester, . .	50,000
"	Mr. Daniel Waldo, of Worcester, .	40,000
"	Mr. Israel Munson, . . .	20,000
"	Mrs. Marsh, ...	10,000

The whole amount expended upon the farm and buildings of the McLean Asylum is about $250,000. It has forty acres of land. The present number of patients is one hundred and seventy-five, although Dr. Bell, its physician, thinks that its apartments are insufficient, comfortably to accommodate more than one hundred and fifty.

During the early part of these proceedings at Boston, measures were taken in New York towards extending the accommodations for the insane. As early as 1815, some of the gentlemen constituting the Board of Governors of the New York Hospital, perceiving the disadvantages of having the Lunatic Asylum connected with that institution, in the midst of a large city, conceived the plan of purchasing a farm in the country, and erecting thereupon, new buildings for the asylum. The late Thomas Eddy, who was untiring in his philanthropic efforts, addressed the board upon the subject, and they authorized the important change to be made. They were enabled to do this, by the munificence of the State

Legislature, which granted to the asylum an annuity of $10,000, for forty years.

A farm on the Bloomingdale road, seven miles from the City Hall, was purchased. The corner-stone of a building was laid on the 17th of May, 1818, and the edifice completed and opened for patients, under the name of "The Bloomingdale Asylum for the Insane," on the 16th of June, 1821. As the number of patients increased, it was found necessary proportionably to extend the accommodations, and perfect the system of classification. Another smaller building was erected, for a portion of the men, in 1829, and a corresponding one for the women, in 1837. The whole cost, to the 1st of Jan. 1839, was nearly $200,000. The farm contains fifty-five acres. The present number of patients is one hundred and forty-five, which is as many as the apartments will render comfortable.

In 1824, at the suggestion of the State Medical Society of Connecticut, a fund of $20,000 was raised by subscription, and this, together with $5,000 given by the state, and $600 by the medical society, was devoted to the purchase of seventeen acres of land, near Hartford, and the construction of a building calculated for fifty insane patients. The edifice was completed and opened on the 1st of April, 1824. It received the designation of "The Retreat for the Insane." Soon afterwards, the state government, willing to aid the institution, granted it a lottery, the charter for which was sold for $40,000. In 1831, the buildings were enlarged at an expense of $10,000, and made capable of receiving ninety patients. The State Legislature made a second appropriation of $5000 in favor of the asylum, and $5000 more was received by individual donations.

About three years since, the subject of the establishment of a state institution for the insane being before the Legislature of Connecticut, a motion was made and prevailed, to send the state patients to the Hartford Retreat, paying that institution five thousand dollars, annually, for their support. With this prospect in view, the establishment has been remodified, improved, and so much enlarged that it now has apartments for two hundred. In 1824, forty-four patients were

received, and in 1845, one hundred and twenty-eight. The present number is one hundred and thirty.

Nearly simultaneously with the first proceedings in Connecticut, the people of Kentucky were awakened to the wants of their insane, and founded a state institution, the buildings of which are pleasantly situated about one mile from the city of Lexington. There are no data within our reach, that might enable us to detail the preliminary proceedings with regard to this asylum. It was opened for the reception of patients in 1824. It appears to have been intended, literally, as an *asylum*, rather than a hospital for curative treatment. No resident medical superintendent was appointed to it until the year 1844. Previously to that time, the medical department was under the care of an attending physician (who made a weekly visit to the asylum), and a young man, resident upon the premises, at a salary of $150 per annum. The condition of the institution may be inferred from its statistics. From 1824 to Dec. 1844, 1,128 patients were admitted. Of these, 404 were cured, 416 died, 93 eloped, and 32 were "removed." Dr. Allen, the present medical superintendent, who made up these statistics, remarks, that in reference to the 404 "recoveries," a very large proportion of them were not stated upon the books to be cures, but that he has reported them as such, because there was *no record of their condition at the time they were discharged.*

The wretched state of the inmates of this asylum, and the glaring imperfections of the establishment, have at length attracted the attention of the Legislature; and a reformation, commenced at the election of its present medical officer, is still in progress. The number of patients at the end of the year 1846, was two hundred and twenty-five.

The State of South Carolina stood nearly side by side, in a chronological point of view, with its sister Kentucky, in the beneficent enterprise of making provision for its insane poor. Her legislature made an appropriation for an institution, in Dec. 1821. A farm of 34 acres, in Columbia, was purchased, suitable buildings erected, and opened for patients in 1827. It is said that owing to some mismanagement of the funds, in the early period of this institution, it became unpopular with

the people, and hence has not been so successful as some others of the kind. The whole cost of the farm and buildings, to the present time, is $125,000, all of which has been defrayed by the state. The farm now contains seventy-five acres. The buildings were intended for one hundred patients. The present number is eighty-three. The principal physician does not reside upon the premises, and, if we properly understand the organization of the institution, takes no part in the executive administration, other than that which may be supposed from an attending physician. Dr. J. W. Parker, is superintendent and resident physician.

The Western Lunatic Asylum, a State Institution at Staunton, Virginia, was founded by an Act of the Legislature, by which $10,000 was at first appropriated for its construction. It was so far completed as to receive patients in July, 1828. Since that time, it has been greatly enlarged. The whole establishment, including a farm of one hundred and ninety acres, cost $110,000, which was paid from the treasury of the state. The buildings are intended for two hundred and thirty patients, and are now fully occupied. The number admitted in 1829, was eight; in 1846, ninety-two. The whole number resident in the asylum during the year 1830, was thirty-seven; and during the year 1846, two hundred and seventy-four.

The medical treatment of the patients at this asylum was intrusted to a visiting physician, until the summer of 1841, when a re-organization took place, and a medical man, Dr. Francis T. Stribling, was made the principal officer.

In the early part of the year 1829, a committee was appointed in the Legislature of Massachusetts, for the purpose of considering the practicability and utility of establishing a state institution for the insane. Further action was taken upon the subject, and in March, 1830, an Act was passed authorizing the construction of a hospital for one hundred and twenty patients. The town of Worcester presented to the state a tract of land containing twelve acres, on condition that the asylum should be erected thereon. In March, 1832, Dr. Saml. B. Woodward was appointed superintendent. The buildings were completed under his supervision; opened, and

the first patient admitted on the 19th of Jan. 1833. The necessity for further accommodations soon became such, that in April, 1835, the legislature authorized the construction of two additional wings. These were soon afterwards erected, making the apartments sufficient for about two hundred and twenty-five patients. The institution, having received a legacy of upwards of $44,000, from Martha Johonnot, has within the last three years again been enlarged.

A building for the most violent patients is now in progress; and, this being completed, the whole cost of the establishment will have been $160,000. Besides the donations already mentioned, there was one of $500, from Mr. McCarthy, of Worcester, for the specific purpose of ornamenting the grounds. Aside from these, the cost of the farm and buildings was paid from the state treasury. The farm now contains one hundred acres. The buildings are calculated for three hundred and fifty-one patients; but the present number is three hundred and ninety-two.

In one of the most comfortless cells of the old county jail, in the town of Worcester, a lunatic, condemned for homicide, had been imprisoned for many years. He had broken a pane of glass in the window, and through the aperture, a bird entered and built her nest in one of the corners of the cell. A stranger visited the jail, and subsequently gave an account of the condition of this maniac, which is said to have been the primary cause of awakening that interest for the insane, in the community of Massachusetts, the result of which was the establishment of the State Lunatic Hospital.

Horace Mann was one of the earliest and most energetic advocates of the hospital, and so ardently did he labor in obtaining the original Act of Incorporation, as well as in forwarding the completion of the institution, that he has not inappropriately been styled its "father."

Vermont followed in the steps of Massachusetts. A Mrs. Marsh having given $10,000, this fund was increased by some smaller donations, and by legislative appropriations, and an institution, called "The Vermont Asylum for the Insane," founded at Brattleboro. It was opened on the 12th of Dec. 1836. During the first few years its patronage was not great;

the number of patients on the 1st of Jan. 1839, being but thirty-six. At a later period, however, the terms of admission were reduced to $100 per annum, and afterwards to $80, and the numbers soon began to increase. The buildings were enlarged, from time to time, and are now sufficiently extensive for three hundred patients. They are at present fully occupied, the number being three hundred and four. The whole amount expended for the farm and buildings is $50,000, all of which, except the donations afore-mentioned, was paid from the treasury of the state. The farm contains ninety acres.

We have no information in regard to the initiatory measures towards the establishment of the Ohio Lunatic Asylum. A farm was purchased about two miles from Columbus, and the buildings commenced in 1836. A large portion of the labor was performed by convicts from the State Penitentiary, and the edifice, being completed, was opened on the 30th of Nov. 1838. It was intended for one hundred and thirty patients. Its apartments were soon filled, and became so crowded, that in the winter of 1842, the legislature authorized the construction of two extensive additional wings. These have been completed, and the institution now has apartments for three hundred and fifty patients. The present number is three hundred and thirty. The expense of the establishment, which was about $150,000, was paid entirely by the state. The quantity of land belonging to the asylum is sixty-five acres.

The year 1839 is memorable for the opening of two institutions for the insane poor; one for the city of New York, and the other for the city of Boston.

The New York City Lunatic Asylum is situated near the northern extremity of Blackwell's Island. When completed, it will be one of the most extensive institutions of the kind in the United States. Hitherto but one wing of the principal edifice has been constructed. In this, and an adjacent building, intended for the most violent, the average number of patients, during the past year, has been upwards of four hundred. An appropriation has recently been made by the

city government, towards carrying out the original plan of the buildings.

In 1836, the Legislature of Massachusetts required the several counties in the state to provide suitable accommodations for such of their idiotic and incurable insane, as could not be received in the hospital at Worcester. The common council of Boston immediately took measures for the establishment of an Asylum for the insane paupers of that city. An edifice 160 feet long by 40 feet wide, and intended for one hundred patients, was commenced in 1837, at South Boston. It was completed at an expense of $32,000, and opened on the 11th of Dec. 1839. Its apartments were soon filled, and have generally been uncomfortably crowded. During the past year, considerable additions have been made, making the capacity of the buildings sufficient for two hundred and twenty patients. The present number is one hundred and forty-seven. The whole cost of the Asylum, $75,000, was paid by the city. There are but five acres of land attached to this institution.

On the 8th of March, 1834, the Legislature of the State of Maine passed an Act, appropriating $20,000 towards the establishment of a State Hospital for the Insane, on condition that a certain sum should be raised by subscription. This condition was soon fulfilled. Ruel Williams, of Augusta, gave $10,000, and Benjamin Brown, of Vassalboro, $3,000. A farm of seventy acres was purchased in the vicinity of Augusta, buildings calculated for one hundred and eighteen patients erected thereupon, and opened on the 14th of Oct. 1840. The whole cost was about $100,000, which was defrayed by the fund already mentioned, and appropriations made by the legislature. The patronage of the institution has gradually increased, until its apartments are fully occupied, and a large addition to the buildings is in progress. It is intended to make the accommodations sufficient for two hundred and seventy-six patients. The farm now contains one hundred acres.

For several years the managers of the Pennsylvania Hospital had looked forward to the building of a distinct establishment, where the patients of the insane department could

be placed under a more judicious and effective treatment than is possible in an institution for general diseases, with limited grounds, and surrounded by a dense population. Having received about $325,000 from the sale of land adjacent to the hospital, a farm of one hundred and eleven acres, about two miles west of the city of Philadelphia, was purchased; and the corner-stone of a building was laid on the 22d of June, 1836. In the autumn of 1840, Dr. Thomas S. Kirkbride was appointed physician to the institution, and under his superintendence, the internal arrangements and organization of the establishment were completed. It was opened for patients on the 1st of Jan. 1841, under the name of "Pennsylvania Hospital for the Insane." In the following year, two detached buildings, for the more violent of either sex, were ordered to be erected. These being completed, the institution had departments for one hundred and eighty patients. Soon after this institution was opened, one hundred and five patients were removed to it from the hospital in the city. A constantly augmenting number being received from the community, the buildings at length became crowded, and in 1846, the detached building for males was considerably enlarged. A similar addition to the corresponding building for females, and a cottage for some of the quiet patients who pay a high price, are now in progress. These being completed, one hundred and twenty patients can be received, and it is intended no further to enlarge the buildings. The present apartments are all occupied, the number of patients being two hundred and two. The cost of the farm and buildings, hitherto, has been $333,000. The expense of the buildings now in progress, will be about $10,000.

The Lunatic Asylum of Tennessee, an institution established at the expense of the state, is in the environs of Nashville, about one and a half miles from the public square in that city. It was opened in 1840, with apartments for about ninety patients. There are but two acres of land belonging to it; but in 1846, a tract of seventy-five acres was leased for the purpose of raising vegetables and furnishing employment for the patients.

The medical treatment of the patients was intrusted to a

visiting physician, until the autumn of 1843, when a resident physician was appointed.

The New Hampshire Asylum for the Insane is in the town of Concord. It was founded entirely at the expense of the state, and cost $43,000. It has a farm of one hundred and twenty-one acres, and beds for one hundred and fourteen patients. It went into operation on the 28th of Oct. 1842. On the 31st of May, 1843, it had forty-seven patients. Its present number is one hundred and two.

An Act providing for the establishment of a State Lunatic Asylum, passed the Legislature of Georgia, in Dec. 1837. The plan afterwards adopted for a building, is that of a principal edifice, 300 feet in length, and two wings, each 129 feet long, parallel with each other, and joining the main building at right angles, at its extremities. A farm of forty acres, two miles south of Milledgville, was purchased, and one of the wings before mentioned erected. This was opened, and the first patient admitted, on the 12th of Oct. 1842. During the first two years of its operations, there were but about forty patients admitted. The other wing has been completed, and the present number of patients is eighty. The principal edifice has not hitherto been commenced. The expenses already incurred amount to about $54,000, and have been wholly defrayed by the state.

In Jan. 1830, Gov. Throop called the attention of the Legislature of New York to the wants of the insane. The subject was subsequently several times before that body, until, in Feb. 1836, the State Medical Society presented a memorial to the Assembly, urging immediate measures for the establishment of an institution. On the 30th of the following month (March), 1836, an Act was passed, authorizing the construction of the State Lunatic Asylum. Trustees were appointed, and a plan adopted for the buildings which, for its magnificence, might challenge comparison with that of any other similar institution in the world. There were to be four buildings, each 550 feet long, and four stories high, placed around a court, and connected at the corners by verandas of lattice work. A farm of one hundred and thirty-three acres, one and a half miles west of Utica, was presented to the state,

by the citizens of that town, and legislative appropriations having been made, the buildings were commenced in 1838. After the completion of one of them, the work was suspended, and the institution opened, Jan. 16th, 1843. Up to the 30th of November, of the same year, two hundred and seventy-six patients had been admitted, and there were remaining, on that day, one hundred and ninety-six.

The necessity of increased accommodations soon becoming evident, the legislature, on the 7th of May, 1844, authorized the abandonment of the original plan, and the enlargement of the establishment, by the erection of two wings, each two hundred and fifty feet in length, connected at right angles with the wings of the original building, and at their other extremities, to be also united by a new building, of smaller dimensions. These additions have been completed, and the institution is now sufficiently large for nearly six hundred patients. The present number is four hundred and forty-four. Excepting the farm, the total amount of expenditure, $431,000, has been paid from the treasury of the state.

In the year 1840, the Sisters of Charity, who had long been connected with the Maryland Hospital, left that institution, and soon afterwards opened the Mount St. Vincent's Hospital, where, for some time, they continued to receive patients. In May, 1844, they purchased the Mount Hope College, removed their patients thither, and gave to the establishment the title of "Mount Hope Hospital." This institution has a department for the insane, and another for those laboring under general diseases. Into the former are received many cases of Mania a Potu. The building was greatly enlarged in 1846, at the close of which year there were sixty patients in the departments for the insane.

A joint committee of the Council and Assembly of New Jersey, was appointed by Gov. Pennington in 1839, for the purpose of ascertaining the number of insane persons in the state, with a view to provide suitable means for their curative treatment. This committee submitted a report on the 12th of Feb. 1840. Another similar committee was appointed, which reported on the 24th of Feb. 1841, and recommended the establishment of a hospital, at the expense of the state.

No active measures, however, were adopted for the accomplishment of this object until 1844, when the Legislature made an initiatory appropriation. A farm, beautifully situated, about two miles north of Trenton, was purchased, and a building soon afterwards commenced. The plan adopted is believed to be superior to that of any of the older structures of the kind in the country. About two months since, Dr. H. A. Buttolph received the appointment of physician to this institution. He commenced his duties on the 1st of May, and is now superintending the completion of the establishment. It will probably be opened for the reception of patients in the course of the ensuing winter.

The late Nicholas Brown, of Providence, Rhode Island, bequeathed the sum of $30,000, towards the foundation of an institution for the insane, in that state. About four years since, the attention of the people was awakened to the subject, and an additional sum of about $100,000 was raised by subscription. Forty thousand dollars of this, was the liberal donation of Cyrus Butler, of Providence; and in honor of this individual, it was determined to designate the institution as the "Butler Hospital for the Insane." A farm of one hundred and twenty acres, about three miles north of the city of Providence, was purchased, and the trustees commissioned Dr. Luther V. Bell, to visit the principal European asylums, and propose a plan for buildings. Having accomplished his object, Dr. Bell returned to this country, and made a report to the trustees, under whose authority he acted, in the summer of 1845. The plan which he proposed is thought to be superior to that of any asylum now in operation in the United States. It was adopted, with perhaps some slight alterations in its details, and the edifice soon afterwards commenced. It is now nearly completed, and will probably be ready to receive patients in the course of the ensuing autumn. Dr. Isaac Ray was appointed physician to the institution, in 1845, and after having visited many of the similar establishments in Europe, went to Providence, where he is now superintending the completion of the buildings.

On the 25th of Dec. 1843, Dr. John Evans, of Attica, Indiana, delivered an address before a committee of the House

of Representatives of that state, urging upon them the necessity of immediately establishing a state institution for the insane. This led to active measures, and a farm of one hundred and sixty acres was purchased in 1845. It is near the town of Indianapolis. Dr. Evans was appointed physician, and commenced the duties of superintending the progress of the establishment on the 1st of Jan. 1846. The building was soon afterwards begun, and is now in progress. Its plan is very similar to that at Trenton. The portion to be first erected is intended for one hundred and fifty patients.

In 1838, a meeting of many of the prominent citizens of Philadelphia was held, for the purpose of considering the propriety of adopting measures for the establishment of an asylum for the insane poor of Pennsylvania. This meeting issued an "Appeal to the People" of the state, in which the necessity and utility of such an institution were ably portrayed In 1840, another appeal was issued from the same source, recommending immediate action on the subject. The Legislature soon afterwards made an appropriation. A farm was purchased on the banks of the Schuylkill, near Philadelphia, and the foundation of a building commenced. No further appropriation being made, the undertaking was abandoned. During the session of 1844–5, the subject was again called up, and an Act was passed, authorizing the purchase of a farm near Harrisburg, and the construction of a building for two hundred and fifty patients. A farm has been procured, but we believe that hitherto the building has not been commenced.

The Legislature of Illinois, at its session of last winter, made an appropriation for the purchase of a site for an asylum for the insane, and accordingly, a farm has been bought at Jacksonville.

The Legislature of Louisiana, also, during its last session, took measures for the establishment of a similar institution in that state, and $10,000 was appropriated for the purchase of a farm, in the town of Jackson.

A department of the general hospital, in Cincinnati, Ohio, as well as one in a similar institution at New Orleans, is devoted to the insane. The same may be said of the Alms

House, at Philadelphia, that of Providence, Rhode Island, and undoubtedly, of many of the pauper establishments throughout the country; but they are so unimportant, so small, or so entirely identified with other branches of the pauper establishments, that they do not come within the limits prescribed for this essay.

Several private institutions for the treatment of the insane have been established in different sections of the United States. The first of these, of which we have any information, was that of Dr. Chaplin, who, some twenty or twenty-five years since, opened an establishment at Cambridgeport, in Massachusetts. This was well patronized, and after the death of Dr. C., it was for some time continued under the direction of his widow.

Dr. Cutter, of Pepperell, Massachusetts, was the next to undertake a similar enterprise. This institution is still in operation, but the number of patients is small.

Dr. Samuel White established a private asylum at Hudson, New York, in 1830. It was opened on the 1st of July, in that year, and during a period of fifteen years, to July 1st, 1845, six hundred and fifteen patients were admitted. Since the death of Dr. White, this institution has been conducted by his son, Dr. G. H. White.

In 1841, Dr. James McDonald opened a private asylum, at Murray Hill, in the city of New York. Having received a liberal and constantly increasing patronage, he purchased an elegant establishment at Flushing, to which he removed his patients, in April, 1845. Here, the number of his patients has continued to increase, and his institution is in that flourishing condition, merited by the superior qualifications and the untiring devotion of its proprietor.

The brief compend of the history of the great philanthropico-scientific enterprise for improving the condition of the insane, would be far from complete were we to neglect to make honorable mention of the exertions of Miss Dorothea L. Dix. During the last six or seven years, this lady has devoted her energies, and the income of a respectable fortune, exclusively to the interests of the insane, and the inmates of prisons and penitentiaries. She commenced her arduous undertaking in

Massachusetts, where she visited nearly every town in the state, inspected the poor-houses, prisons, and asylums for the insane, and sought out those many victims of insanity who were kept in miserable private receptacles, cellars, and dungeons. Since that time she has traversed, in a similar manner, the states of Rhode Island, New York, New Jersey, Pennsylvania, Indiana, Kentucky, and some others; has visited most of the Southern States; and has recently returned, with impaired health, from these laborious duties in the west. She has presented memorials to the legislatures of several of the states, in which she has vividly portrayed the condition of the insane within the territories severally under their legislative jurisdiction. The establishment of several of the institutions for the insane was in a great measure the result of her exertions.

Of all the physicians who have acted as medical superintendents to public institutions for the insane, in this country, Drs. Wyman, Todd, and Lee, are the only ones who are not still living. A brief notice of these, as well as of Dr. White, the founder of a private institution, may not be inappropriate.

Dr. Rufus Wyman was appointed in 1818, physician and superintendent of the McLean Asylum, near Boston. He was the first physician in the United States who was made the resident superintending officer of an institution of the kind, and is said to have been "most happily adapted to the exigency then present of laying broad, deep, and firm this class of public charities," in New England. His insight into human character, his knowledge of medicine and metaphysics, his philanthropy, and last, though far from least, his undeviating attention to the welfare of his patients, admirably qualified him for his situation. It is said that for seventeen years, and until his health yielded under the weight of his exertions, he was scarcely absent from the hearing and sight of the insane. "There was a moral beauty in his character," says Dr. Bell, "a sterling, uncompromising integrity in him, as a director of a public institution, which may well serve as a model to all who may be called upon to discharge such sacred functions. In his relations to insanity, he was emphatically a just man; honest, not only in the great open

duties of his station, but in those more minute and scientific details wherein a deviation, or looseness, as regards exactitude, any partial coloring or omission, could only be known to himself, or incidentally to those who might come after him in his duties. He had such distrust of any form of reputation higher with the public at large than with his professional brethren, that perhaps the light of his good works was too much hidden under a bushel to serve its proper end as an example." He resigned his situation in May, 1835, and died in 1842.

Dr. Eli Todd was born in New Haven, Connecticut, in 1768. He graduated at Yale College, in 1787, studied medicine in his native town, and afterwards settled at Farmington, a town adjourning Hartford, on the west. Here he became eminent in his profession, and was elected President of the State Medical Society.

In 1819, he removed to Hartford, and upon the opening of the Retreat for the Insane in that city, was appointed as the superintendent of that institution.

The Retreat in a great measure owed its existence to his exertions, and under his judicious administration, it became, perhaps, the most popular institution of the kind in the country. His system of moral regimen was founded upon enlightened principles, and closely approximated that which is pursued at the present time, in the best establishments. In 1833, his health became so much impaired, that during the summer, he travelled in the hope of restoration. In this he was disappointed. He continued to decline, and died on the 17th of November.

Dr. Todd was much esteemed as a citizen, and beloved for his social and generous disposition. Dr. Brigham alludes to him in the following terms: "He possessed," as Spurzheim said, after seeing him, "a mountain of benevolence," to which were added a good education, fine personal appearance, most engaging manners, and very superior conversational powers, all of which eminently fitted him for the moral treatment of the insane, in which he particularly excelled. His great merit, we conceive, is his having zealously embraced, and practically introduced into this country, and made extensively

known here, the moral and medical treatment recommended by Pinel, Luke, and Willis.

Dr. Thos. G. Lee succeeded Dr. Rufus Wyman, as physician and superintendent of the McLean Asylum. He commenced his duties in May, 1835, and continued in the active and energetic performance of them, until the autumn of the ensuing year. His health then failed, and he died on the 30th of Oct. 1836.

At a meeting of the board of trustees of the institution with which he was connected, several resolutions were passed, of which the following contains a beautiful tribute to his character and memory.

Voted, "That the board, while submitting in sorrow to the dispensations of Providence, cannot but feel deeply the loss which the institution under their care, and the public, have suffered in the lamented death of Dr. Lee. They had known him long enough to appreciate his talents, his attainments in his profession, his remarkable and entire devotion to the pursuit in which he had engaged, the beautiful purity of his character, the elevation of his views, and the propriety of the means by which he sought to attain the most worthy objects. They have often been struck with the soundness of his judgment, and the kindness of his manners; and have perceived in the institution of which he was superintendent, the happy influence of his professional skill, combined with the cheerfulness and gentleness of his deportment, and the piety which was the habitual guide of his life. After an association of nearly two years of an intimate character, they can say with truth, that they have nothing to regret in their intercourse with him, but its premature close. They had hoped to see the McLean Asylum long increasing in usefulness under his care, and to witness the extension of his well-earned reputation for many years; and they cannot suffer him to pass to the grave, without paying a just tribute to his many admirable qualities, and his peculiar fitness for the station in which he was placed."

Samuel White, M. D., was born in Coventry, Connecticut, on the 23d of Feb. 1777. He pursued his medical studies with Dr. Philip Turner, of Norwich, Connecticut; settled in

Hudson, New York, in 1797, where his practice soon became extensive; and for more than a quarter of a century before his death, was eminent as a physician and surgeon, esteemed as a good citizen, and respected for his many Christian virtues.

He received the appointment of Professor of Obstetrics and Practical Surgery, in the Berkshire Medical Institute, at Pittsfield, Mass., in 1828. After having fulfilled his duties here, for two years, he resigned.

In 1830, he established the Hudson Lunatic Asylum, which, under his direction, soon became the most extensive and popular private institution for the insane, then existing in the country. In 1843, he was elected to the office of President of the New York State Medical Society, and his address delivered before this association is one of the best brief compends of the treatment of insanity ever published. In Oct. 1844, he was elected as Vice-President of the Association of Medical Superintendents of American Institutions for the Insane. He died at Hudson, on the 10th of Feb. 1845.

Bloomingdale, New York, June, 1847.

LIST OF PHYSICIANS OF THE INSTITUTIONS FOR THE INSANE IN THE UNITED STATES, WITH THE DATES OF THEIR ELECTION, &c., &c.

Physicians.	Date of Election.	Institution.	Remarks.	Relinquished office.
Rufus Wyman, M. D.	1817,	McLean Asylum for the Insane,		May, 1835.
Eli Todd, M. D.	1824,	Retreat for the Insane, Hartford,		Died in office, Nov. 17th, 1833.
James McDonald, M. D.	1831,	Bloomingdale Asylum for the Insane,	Commenced duties Oct. 13th, 1832,	August 15th, 1837.
Samuel B Woodward, M.D.	1832,	Massachusetts State Lunatic Hospital,	Superintended erection of buildings,	July 1st, 1845.
Silas Fuller, M. D.	1834,	Retreat for the Insane, Hartford,	Succeeded Dr. Todd in June, 1834,	January, 1840.
William H. Stokes, M. D.	1834,	Maryland Hospital.	The first resident physician,	September 30th, 1835.
Thomas G. Lee, M. D.	1835,	McLean Asylum for the Insane,	Succeeded Dr. Wyman,	Died in office, Oct. 30th, 1836.
William Fisher,* M. D.	1835,	Maryland Hospital,	Succeeded Dr Stokes, Oct. 1st, 1835,	June 30th, 1846.
William H. Rockwell, M. D.	1836,	New Hampshire Asylum for the Insane,		Continues in office.
Luther V. Bell, M. D.	1836,	McLean Asylum for the Insane,	Commenced duties Jan. 1st, 1827,	Continues in office.
Benjamin Ogden, M. D.	1837,	Bloomingdale Asylum for the Insane,	Succeeded Dr. McDonald,	September 16th, 1839.
William M. Awl, M. D.	1838,	Ohio State Lunatic Asylum,	Superintended erection of buildings,	Continues in office.
William Wilson, M. D.	1839,	Bloomingdale Asylum for the Insane,	Succeeded Dr. Ogden,	April 1st, 1844.
John S. Butler, M. D.	1839,	Boston Lunatic Asylum,		October, 1842.
Amariah Brigham, M. D.	1840,	Retreat for the Insane, Hartford,	Succeeded Dr. Fuller in July, 1840,	August, 1842.
Cyrus Knapp, M. D.	1840,	Maine Insane Hospital,		April 14th, 1841.
Thomas Kirkbride, M. D.	1840,	Pennsylvania Hospital for the Insane,	Superintended completion of buildings,	Continues in office.
Isaac Ray, M. D.	1841,	Maine Insane Hospital,	Succeeded Dr. Knapp, Aug. 12th, 1841,	April 11th, 1845.
Francis T. Stribling, M. D.	1841,	Western Asylum, Virginia,	Had been visiting physician from 1836,	Continues in office.
John M. Galt, M. D.	1841,	Eastern Asylum, Virginia,		Continues in office.
George Chandler, M. D.	1842,	New Hampshire Asylum for the Insane,	Superintended completion of buildings,	August 26th, 1845.
Charles H. Stedman, M. D.	1842,	Boston Lunatic Asylum,	Succeeded Dr. Butler,	Continues in office.
Amariah Brigham, M. D.	1842,	New York State Lunatic Asylum,	Superintended completion of buildings,	Continues in office.
John S. Butler, M. D.	1843,	Retreat for the Insane, Hartford,	Succeeded Dr. Brigham in May, 1843,	Continues in office.
David Cooper, M. D.	1843,	Georgia Lunatic Asylum,	Commenced duties in Jan. 1844,	January, 1846.
John S. McNairy, M. D.	1843	Tennessee Lunatic Asylum,		Continues in office.
John R. Allen, M. D.	1844,	Kentucky Lunatic Asylum,		Continues in office.
Pliny Earle, M. D.	1844,	Bloomingdale Asylum for the Insane,	Succeeded Dr. Wilson,	Continues in office.
James Bates, M. D.	1845,	Maine Insane Hospital,	Succeeded Dr. Ray,	Continues in office.
Andrew McFarland, M. D.	1845,	New Hampshire Asylum for the Insane,	Succeeded Dr. Chandler,	Continues in office.
Isaac Ray, M. D.	1845,	Butler Hospital for the Insane,	Is now superintending erection of buildings,	Continues in office.
John Evans, M. D.	1845,	Indiana State Hospital,	Is now superintending erection of buildings,	Continues in office.
Thomas F. Green, M. D.	1845,	Georgia Lunatic Asylum,	Succeeded Dr. Cooper, Jan. 1846,	Continues in office.
George Chandler, M. D.	1846,	Massachusetts State Lunatic Hospital,	Succeeded Dr. Woodward,	Continues in office.
John Fonerden, M D.	1846,	Maryland Hospital,	Succeeded Dr. Fisher,	Continues in office.
H. A. Buttolph, M. D.	1847,	New Jersey State Lunatic Asylum,	Superintending completion of buildings,	Continues in office.

* Hezekiah Starr, M. D., occupied the place of Dr. Fisher from Jan. 1st to Sept. 30th, 1840.

REPORT

OF A COMMITTEE APPOINTED BY THE ACADEMY OF MEDICINE,

UPON THE

COMPARATIVE VALUE OF MILK.

FORMED FROM

THE SLOP OF DISTILLERIES AND OTHER FOOD.

By Augustus K. Gardner, M. D., *Chairman.*

READ MARCH 1, 1848.

Your committee appointed under the following resolution,

Resolved, That a committee of eleven be appointed by the Academy, to examine into the effects, proximate and remote, upon the general health of the city, caused by the numerous distilleries, gas works, slaughter houses, milk establishments, lead manufactories, tanneries, and all other manufactories and establishments which are, or may be styled nuisances, from their deleterious effects upon the health; and particularly to report the effect upon the human economy of milk taken from kine, tuberculously or otherwise diseased, from improper food, or confinement in these milk establishments:

have, in commencing their labors, attended to that part of their duties laid down in the following extract from the resolution, "And particularly to report the effect upon the human economy of milk taken from kine, tuberculously or otherwise diseased, from improper food, or confinement in these milk establishments."

In investigating this matter, your committee have not forgotten the importance of careful attention, accurate and repeated examinations, unswayed by any bias. The importance of such an investigation, of an article which constitutes so large a portion of the food of the inhabitants of this city, is evident to all. In 1842, it was estimated that the quantity of milk, daily used in the city of New York, amounted to more than 15,000 gallons, at that time principally furnished from the neighborhood. With the growth of the city, the consumption of milk has undoubtedly increased. The immediate vicinity is no longer competent to furnish the quantity demanded, and it now comes to us from long distances, brought hither by the agency of the great motor principle of the age—steam. The various kinds of milk which are consumed in the city may, therefore, be divided into several classes, according to its origin.

1*st.* The grass-fed milk, brought from a distance by steam —as the Orange County milk, for example.

2*d.* That produced by feeding the cows entirely, or partially, with distillery slop—which is the refuse of the grain, left after distillation, mixed with water, with most or all of the spirituous ingredients extracted, brought hither by steam—as for example the Newburg.

3*d.* Grass-fed milk produced in the neighborhood of the city, as some of the Bloomingdale dairy milk.

4*th.* That produced partially by the administration of distillery slop in the neighborhood, as at Brooklyn, Wallabout, Bloomingdale, &c.

5*th.* That produced in the city, or on its outskirts, solely by feeding on distillery slop—as at the distilleries on Long Island, Sixteenth and Forty-Second Streets in this city.

No. 1, is from cows as nearly as possible in a state of nature, nourished by natural food, and unconfined, having that exercise and pure air so necessary for health.

No. 2, is from cows that are in some respects fed and kept like the former, but whose secretive organs are stimulated to unnatural action by distillery slops, or the grains from breweries, and whose winter nourishment is said to consist principally of this food.

No. 3, is like number one, except that it is brought to market by milk carts.

No. 4, is similar to No. 2, with the same reservation as before.

No. 5, is from cows kept in, or near the city, constantly confined in stables, and fed entirely, or nearly so, on distillery slop. These cows are kept in low sheds, sometimes at a distance from, but generally near a distillery. A description as summary as possible of the cow-sheds attached to Johnson's distillery, corner of Sixteenth Street and Tenth Avenue, will, perhaps, give as good an idea of this method as can be stated in any manner. This collection of pens is selected for description, on account of its size and neatness, being far superior to any of the others in both these particulars.

The lofty chimneys of the distillery, constantly vomiting forth its black and sulphurous smoke, will guide your steps thither. Should the wind blow towards you, the olfactory organs will, while yet at a distance, aid your researches. It extends from street to street, and between it and the Tenth Avenue, are the cow-sheds. Cross the avenue, and the whole block, to the very edge of the water, is filled with continuous rows of sheds, separated from one another by paved roads, where stand the milk carts and the cans. Enter them, and internally these sheds are sub-divided into apartments sufficiently large to accommodate two rows, of twelve cows each, standing tail to tail. They are slightly raised from the level of the floor, by an inclined plane, and are tied by cords, or leather straps which pass around the neck. Here they stand by day and sleep by night, without bedding, upon the wet floors. Between the rows is a narrow space, so inclined that the urine, &c., will flow off easily. Running by their heads is a long trough to contain the slops, or swill, as it is sometimes called. Without the building, at proper intervals, are tanks containing the "slop" brought down from the distillery, through conduits, running under the street, and so constructed that by removing a plug, it will flow into the troughs immediately before the cattle,—thus saving the great labor of transportation, necessary when the stables are not contiguous to the distillery.

It is not easy to compute the number of cows kept here, but the lowest estimate is two thousand, the highest four thousand. They commence their city life in the spring, when the stock is almost entirely renewed, those of last year being fatted and carried to the shambles. They come from the country soon after calving, and are immediately tied up in these stalls. The calf is soon killed. For nutriment they are offered this slop, which they not unfrequently refuse to drink for a day or two. Starvation is their only alternative. It is, indeed, rather offensive with its peculiar, half sour, half spirituous odor, as it comes bubbling, foaming, and steaming, from the tanks.

The heat of this liquor is so great that not unfrequently it is served to them when it would scald the finger placed in it. The cattle nearest to the tank, even when inured to it, draw back from the heat of the fumes; the next, excited by appetite, carefully lap it with the ends of their tongues, hardened by frequent exposures of this kind; while those further along are enabled to drink it as it comes cooled by the passage.

As the toper or the habitual user of tobacco is wedded to his deleterious practices, so is the poor cow to this "slop." She soon learns to like it, and awaits the time for its distribution with manifest anxiety. She drinks herself full. Soon after she is given her pittance of hay, oil-cake, or bran. This is not customary in all establishments. Nature requires a certain quantity of bulk, and she eats a little, but with slight relish. From the day of her entrance into this Bastille, she is not allowed a single draught of pure water. She soon lies down, or mayhap remains standing in a very stupid state. This may be from repletion, but more probably from a small quantity of alcohol remaining in the "slop," for she seems drunken. When she lies down, it is not in the easy position of the cow in the pasture, with her feet folded under her. She is rather lying on her side, with outstretched neck and feet, resembling the uncomfortable position in which the besotted drunkard throws himself.

The eye so meek and gentle that Homer dignified the queen of the gods by the epithet βοωπις, or the "ox-eyed," has here a staring, stupid gaze, though immediately before

feeding, the excitement renders it preternaturally brilliant. They seem to suffer but little, for the constant stupor which overshadows them, apparently dissipates all feeling. They make no attempt at resistance, braving calmly whatever is inflicted upon them, scarcely rising by kicks and blows; and the cow formerly fractious and unruly, may here be taken by the nose, and the mouth opened without manifest objection—things almost impossible in a common stable. These cows are attended with much care, frequently curried, the stables kept as clean as possible generally. At other establishments, as in one near Forty-Second Street, the cows are neglected and the stables shockingly filthy—truly Augean stables, the scent of which is perceptible to a very great distance.

A few days after their arrival, disease not unfrequently attacks them, and often with such virulence that it is not uncommon for the animal, which cost $40, to be dead at the expiration of a fortnight after her entrance, benefiting her owner but little more than the value of her skin.

One of the most common and early appearing diseases, is what is called by the cowherds "sore-foot." What the disease is your committee cannot state. The symptoms and pathological condition are as follow.

After a longer or shorter residence in these stables, the first symptom noticed, is the tenderness of one or more feet, most commonly (as we gather from the keepers) the front-feet. This tenderness goes on increasing, accompanied by a swelling, which is generally local, situated around the hoof, but occasionally spreading up the leg. Sometimes but one foot is attacked—by foot we refer not to the foot as commonly designated, but rather to the anatomical division—more generally two, and sometimes all four. The pain soon becomes great, and the animal is no longer able to bear the weight of her body upon them. After this, she rises, if at all, with great reluctance.

But little curative means are used. While all confess the efficacy of pasturage as a cure, it is rarely tried, for the disease, as a general thing, must be endured by every animal, sooner or later, before they become acclimated. Cold water is sometimes used both as preventive and remedy. This is

administered by filling the narrow passage between the rows of cows with water, and by drawing back they are enabled to stand with their hinder feet in it. Heat is generally considered to be the cause of the complaint, and when sick they are occasionally allowed to go out and breathe the fresh air. If the attack is slight, they are fed and milked as usual. If more severe, they are fed with bran, hay, &c., and the milking continued as ever. Some owners say that they throw away the milk from cows thus diseased, it being drawn only that the secretion may not be arrested. Others allow that they are less scrupulous; some even confessing that those thus affected give far more, and better milk than when well.

In several cases, which your committee have examined, there was found after death, a fungus growing out between the hoofs; several external orifices discharged a fetid pus; while within, the bones were carious, the ligaments ulcerated, and the whole limb quite disorganized. Your committee in some instances have seen pus in the cellular tissue, extending up the foot.

A peculiar disease, not found when the animal is kept in a healthy situation and fed with natural food, is a caries or absorption of the teeth. This is found most generally in the mouths of animals that have been kept in the stable for six months. They seem to be absorbed, appearing very much as one would suppose they might, had they been exposed to the effect of a strong acid. The general appearance is as if the crown of the teeth had been entirely decayed, leaving a yellow, irregular stump or fang, less in size than the natural, as if that, too, had been partially absorbed. This fang is loose, moving easily under the finger, and in not a very long time falls out. There is little or no active inflammation around the teeth of the animals while alive, and apparently little pain, as they eat hay with them, though necessarily slowly. After death the alveolar processes are often found entirely absorbed. The maxillary glands are not generally enlarged.

The reasons given by those engaged about the cows for this peculiar disease, are quite various. Some ascribe it to age. This is obviously incorrect, as the disease is seen in young cows equally with the old. Some give the extreme heat of

the "slop" as the cause, while others suppose the origin to be either the transitions from heat to cold, or the characteristic principles of the food. The probability is that all these three latter are essential agents in producing this disease. This opinion will be apparent from the following result of their observations. The disease appears to be mainly confined to those teeth most exposed to the heat—the front teeth—while the grinders equally exposed to the chemical causes remain quite sound, when the incisors are completely destroyed. It is stated that the teeth of those animals which are kept at a distance from the distillery, and where the food is necessarily cooled in its transportation, are less affected. These views, however, remain to be verified by future observations. True it is that from some cause or other, the teeth of cattle, thus situated and fed, are prematurely lost. It may be from this circumstance, that the horses employed in carrying out the milk, though kept in the same stables, are never fed upon this food, called so nutritious.

Another peculiarity is the "elongated hoof." This has been considered a disease, but in the opinion of your committee it is merely an elongation of the hoof from natural causes. This part of the animal, like every other, has a growth proportionate to its wants. In a natural state, the beast is walking and running about during the whole day, frequently upon rough and stony roads. This exercise wears down the hoof to the proper length; but confined in the stable for months, the growth continues without the wear which would reduce it to the ordinary dimensions. It is for this simple reason that the hoofs of these animals are sometimes seen 8 or 10 inches in length, and preventing the animal from standing with ease. A similar occurrence is not unfrequently seen in caged birds, requiring, as with the cows, amputation. As well might the elongated finger-nails of the Chinese nobility be called diseased, for they are not unfrequently several inches in length, and kept in gold cases to protect them from being broken. Any work must destroy them, and it is only as an evidence of a life of ease that they are prized.

Besides these minor evils, there are others of greater magnitude. Not unfrequently the death of these cows is extremely

sudden. We have been informed by a dealer, that he has known a cow, apparently in good health, die while she was being milked, crushing in the fall the dairyman under her, and so confining him, that he could not rise without aid. What was the complaint, he knew not.

Various other diseases affect them, some of which perhaps are not more frequent than among the same number elsewhere. In these establishments, however, they are regularly milked, notwithstanding their malady. We have seen at the stables in 16th street, one affected with hernia, another with a large fluctuating abscess on the buttock, apparently of sufficient size to contain a gallon of fluid. The teat of one that was accidentally cut was milked while the healing process was going on. Dr. Cock, jr., says that he has seen the genuine vaccine disease among them. Serious injuries from bruises, &c., are not of unfrequent occurrence.

During the months of August, September, and October, 1847, an epidemic prevailed to a very fearful extent, in the pens of the distilleries, more than decimating their numbers. The first evidence of its attack, was the diminution of milk, decreasing a half in twelve hours. Its progress was then fearfully rapid; twenty-four hours frequently witnessed the approach of the disease and the death of the animal.

The treatment was various, and with equal success. Bloodletting and purging, which the symptoms and the pathology of the disease indicated, were useless, for few if any had the constitution to stand against it. Equally so were blistering and rowelling. Conscious that their confined state is an artificial one, the keepers generally limited their treatment to an attempt to restore them to more healthy influences. They were released from their stall, allowed to breathe fresh air, their slop removed, and its place supplied by meal, hay, and bran mashes. Bedding was placed under them, thus alleviating their exit from the world. The change from their heated stable to a cooler atmosphere not unfrequently aggravated the disease.

Your committee made post-mortem examinations of a number of these animals, and found always the same pathological changes. The animals were some of them long residents in

these stables, which was known by their hoofs ; others apparently more recent comers. The skin was lustrous like the coat of a highly groomed horse, and the animals appeared well conditioned. On opening them, but little flesh was found to be upon them, their bloated state being the cause of their apparent good order. Scarcely a pound of fat could be found. The omentum, which in milch cows would generally vary from 20 to 25 pounds, in these animals would not weigh a pound. This was universally the case as far as we have seen, and it is reported to be always so in distillery fed animals. The stomach and intestines, as far as examined, were healthy.

On opening the thorax, the whole cavity sometimes was filled with coagulable lymph and serum, but generally only one chest was thus affected. In several instances, the coagulable lymph and the serum appeared thrown out in layers, the first forming as it were a sponge, which was fully absorbed with the serum. The quantity of lymph was so great, that it was at first taken for fat—a mistake which the similarity in color contributed in making. There were frequently many gallons of this effusion. In some instances the pleura pulmonalis was closely attached to the pleura costalis by strong and recent adhesions. In every instance, combined with the pleuritis, was an active inflammation of the cellular tissue of the lung itself, affecting sometimes one and sometimes both, to greater or less extent. This was seen in various stages; one lung frequently exhibiting all the phenomena of pneumonia remarkably distinct, from the simple inflammation of a single lobe, to a general hepatization of the lung, so that it would sink entire in water; or still further, in the state of soft degeneration. One of these lungs is reported to have weighed 42 lbs. Save in one, single, doubtful instance, there was seen no trace of tubercle. As far as your committee could gather from those who are habitually employed in opening the dead animals, and turning them to the best possible use, tubercular deposits are not common, though they are occasionally seen. Their statement is of but little value, as it is doubtful if they know what is meant by tubercle.

Some, possibly trivial, facts in the appearance of these animals have struck the attention of your committee, namely,

1*st.* The extreme brilliancy and unnatural wildness of the eye (which appearance alternates with the "besotted and staring" expression before mentioned), and the general lethargy of the body, and apparent absence of fear and anger.

2*d.* The unpleasant smell of the breath, generally so sweet and agreeable, now reminding one of an old beer-bottle.

3*d.* The immense quantity of urine, and small quantity of fæces evacuated daily,—the colorless appearance of the urine, and the want of consistency in the fæces, being softer than the dung of grass fed animals.

4*th.* The manner in which the urine and fæces are discharged, standing in awkward positions, not unfrequently while reclining, and the urine not flowing in a continuous stream, as is natural with this animal, but *per saltum*, by jets. The quantity of the urine may be imagined when informed that thirty-two gallons of slop is the daily allowance for each cow, and that ten quarts of milk per day is a large average yield.

5*th.* They are very rarely seen chewing the cud. This arises from their food not requiring mastication.

6*th.* Their difficulty in breathing during the heats of summer—constantly panting as if they had been hard driven.

Reasoning *à priori* in regard to the whole matter, considering the facts, that several thousand cows are kept confined in a small space, deprived of all exercise—some hundreds under one roof but a few feet above their heads, in summer, heated to an intensity almost suffocating by the direct rays of the sun, by the steam from the boiling slop, by their own breath, and the exhalations from their bodies, constantly damp from the vapor arising from their food, and the floors wet with the ever flowing urine, standing and lying on hard boards, without litter by day or night; fed on food, if not injurious in itself, certainly ill adapted to the formation of their stomachs, which need not possess so complicated an apparatus as the rumen, reticulum, and omasum, when the latter could easily fulfil all the duties required to digest this species of food—one would most surely arrive at the conclusion that these animals could not give natural and healthy milk.

Your committee, having taken the best means in their

power to examine into this matter, now present to you the following results.

In examining this subject your committee have been, in a great measure, compelled to make the road themselves, for no track was found opened before them. There has been no report seen of any European investigations of diseased milk. The treatise by Robert M. Hartley, Esq., of this city, a work of great merit, was written to sustain an opinion; and like all treatises on new topics, contains errors either arising from limited or hasty investigations, or from unscientific deductions drawn from correct data.

Your committee have endeavored to take nothing for granted, but have striven to verify by repeated observations, the truth or falsity of every statement, and endeavored to guard against any fraud. For this reason, every specimen of milk examined, has been personally obtained by the chairman of this committee, from the milkers themselves, and placed in the hands of the chemist. Any adulterations and additions have been made, therefore, by the producers themselves, and are not attributable to any of the retail venders. The distillery milk has all been obtained from Johnson's Distillery, Sixteenth Street and Tenth Avenue. The Orange County milk was bought at the depôt, at the foot of Duane Street, without the knowledge of the proprietors of the object for which it was procured, and taken from the same can from which other customers were served. It, therefore, was not enriched with additional cream for the purpose.

"The purity and richness of milk," says a recent standard writer,* "were formerly estimated by its specific gravity, which is about 1.032; if the milk was diluted with water, it was supposed that the gravity of the fluid would be in the first case increased, and in the second lessened.

"The cream being the lightest element of the milk, its deficiency or abstraction would, of course, increase the density of the remaining fluid; and the addition of water, after the removal of the cream, which is also of less weight than milk which is even pure and rich, would of course raise the

* Arthur Hill Hassall.

gravity of the milk either up to, or beyond its natural weight." This method of analysis has, therefore, been rejected.

The chemical examinations were intrusted to Mr. Lawrence Reid, whose skill as an analytical chemist is well known to the members of the Academy. He has devoted much time and attention to the investigation, proposing and using new tests of its virtues, some of which will be hereafter mentioned, and displaying that zeal in the cause of science, which strongly recommends him to the favorable consideration and thanks of the Academy.

For the microscopical investigations, the Academy is indebted to the practised eye and sound judgment of Dr. Alonzo Clark, Professor of Pathology in the College of Physicians and Surgeons, in this city. For his readiness in so materially assisting in this investigation, Dr. Clark will undoubtedly receive the acknowledgments of the Academy, as he has already the personal thanks of the committee.

The reports of these gentlemen now follow in full.

New York Hospital, *Nov. 29th*, 1847.

Sir:—At the request of the committee of the Academy of Medicine on milk, I have made several analyses of distillery and other milks, and have the honor to report:

1st. That of the various samples furnished me, none of the common sophistications were present, such as starch or carbonate of lime, and that as far as chemical tests could enable me to determine, the samples were all genuine.

2d. That with the exception of the sample marked No. 6 in the analysis they were all of a neutral character when received, No. 6 being acid.

3d. With regard to the assertion that distillery milk contains spirits, the milk has been tested on such a scale as to detect one fifty thousandth part of spirits if present, and none could be found.

4th. With regard to distillery milk furnishing no butter, it is a mistake, as every sample, sixteen, gave butter on agitation in a bottle. The butter is whiter and in much smaller quantity than obtained from other milk, and this may be considered a good test of distillery milk, the scantiness of the

butter. The butter also, in forming, associates itself with more curd and whey than that obtained from other milk. To examine milk for butter, a bottle containing a pint may be filled two thirds with milk, and well shaken for about one hour and a half, when the butter will separate.

5th. With regard to the relative proportions in which the ingredients exist. The nature of fermentation and distillation is to abstract from the grain all the fecula and sugar, the principles that are more particularly convertible into butter and sugar; leaving the nitrogenized compounds and also the caseine and earthy matter nearly untouched; hence the increased quantity of ashes and also of caseine, the nitrogenized compound in milk; while the sugar and butter are below the usual standard.

6th. To test if this milk was decomposed by heat in the same manner and time as other milk, a portion was placed in a glass vessel and retained at the temperature of 98 degrees of heat for six hours before coagulation took place, while a portion of Orange County milk treated in the same manner coagulated in one hour.

Whether this experiment is to be considered as having a bearing on the assimilation of the milk as food in the human stomach, I prefer leaving to the committee to decide.

The samples are marked from one to six. No. 1 is a European analysis of milk by M. Haidlen, the most recent I could find. No. 2 is Orange County milk, being an average of two samples. Nos. 3, 4, 5, 6, are distillery milk, about sixteen samples of which were examined.

Yours respectfully,

LAWRENCE REID,

Prof. of Chemistry to College of Pharmacy.

A. K. GARDNER, M. D.,

Chairman of Committee of the Academy of Medicine on Nuisances.

ANALYSIS OF MILK.

	No. 1.	No. 2.	No. 3.	No. 4.	No. 5.	No. 6.
Water, - - -	873.00	860.00	869.10	876.00	888.00	898.00
Butter, - -	30.00	35.00	15.00	14.00	13.00	10.00
Caseine, - - -	48.20	45.00	62.00	59.00	50.00	45.00
Sugar of Milk, -	43.90	53.00	44.00	42.00	41.00	40.00
Phosphate of Lime, -	2.31	3.35	4.20	4.00	3.20	2.80
" Magnesia,	.42	.76	1.84	1.56	1.41	1.20
" Iron,	.07	.09	.12	.11	.10	.07
Chloride of Potassium,	1.44	2.00	2.97	2.51	2.46	2.23
" Sodium,	.24	.36	.44	.42	.43	.40
Soda in combination with the caseine,	.42	.50	.43	.40	.40	.30
	1000.	1000.	1000.	1000.	1000.	1000.

Dear Doctor :—I have examined, with the aid of the microscope, the four specimens of milk you sent me. Four statements will comprise all that I observe in them worthy of remark :

1st. The milk-globules are less abundant in them than in specimens of good country milk, with which they have been compared.

2d. The globules, though very variable in size, are generally smaller than in good milk.

3d. In three of the specimens there was an unusual tendency to aggregation in the globules, and when once aggregated they adhered in groups, so that mere agitation would not separate them.

4th. In the last two specimens there was an unusual number of epithelial cells,* many of which were very markedly granular, and some highly colored—in a few of these the milk-

* " The cells covering the simple membranes that form the free surfaces of the body, whether external or internal, are all entitled to be regarded as *secreting* cells ; since they separate from the blood various products which are not again to be returned to it." (Carpenter's " Manual of Physiology," p. 408.)

" From the researches of Mr. Goodsir, it appears that in common with other glandular structures, the inner surface of the milk-follicles is covered with epithelial cells, which being seen to contain milk-globules, may be without doubt regarded as the real agents in the secreting process." (Carpenter's Physiology, Clymer's ed., 1847. P. 649—see also " Anat. and Path. Obs.," by John and Harry D. S. Goodsir, p. 24.)

globules were still imprisoned, and of very small size ; showing that these secreting structures had been discharged from the lactiferous ducts of which they form the lining, before the complete elaboration of their contents.

Your familiarity with the subject to which these statements relate, will render any comment on my part unnecessary. I submit them, therefore, as the basis of any remark which your wider range of research may justify.

Your ob't servant,

A. Clark.

New York, Nov. 27, 1847.

To Dr. Gardner.

There are some persons who have attempted to deny that the health of the animal, or the food taken, can possibly affect the milk. The statement is so easily proved to be erroneous, that your committee would not think of noticing it, were it not urged so strenuously, that they feel compelled to give it a passing remark.

It is a settled fact that many articles taken as food or medicine, into the stomach, have been detected in the milk a few moments after. The coloring matter of madder root, the odorous qualities of garlic and turpentine, neutral salts, nitrate of potassa, have all been found in the milk of women ;* the color and smell of carrots in the milk of an ass.† Cow's milk is well known to be so sensibly affected by carrots, turnips, onions, cabbages, as to be perceptible to the taste. It frequently possesses a bitter principle, when the animal has been grazing among young birch or other trees. That the properties, as well as the smell of various substances, are preserved, is evident from the common custom of purging the child by giving castor-oil to the mother.

Every medical man knows that the lips of children nursed by a woman affected with syphilis, are frequently rendered

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* Microscopic Anatomy of the Human Body, by Arthur Hassall, London, 1846.

† Experiments of Péligot, "Cours de Microscopie," by Al. Donné, p. 454. Paris, 1844.

sore, and the whole constitution tainted with this loathsome malady. Yet Mons. Donné states that he has never been able to detect any change in the character of the milk in such cases. This instance is not a solitary one, for we all know that children are born affected with syphilis inherited from the father; the poison thus passing through the systems of both parents, the child being diseased and the mother escaping free.

Numerous statements have been made orally and written, by men of science and veracity, testifying to the marked effects of the slop-fed milk upon children, and the cessation of all disturbance and the restoration to health, immediately upon discontinuing the milk; and numerous deaths, which under the head of Marasmus swell the lists of mortality, are ascribed in great measure to this cause.

The common opinion is, that the milk is injurious. The keepers of the cows, the ignorant stable boys (who are composed of the most stupid Irish and Germans to be found), and the residents in the neighborhood, will not confess the use, or their willingness to drink this milk. One man who is employed upon these animals when dead, says, "no money would tempt him to drink the milk constantly, for cows that have such insides cannot give healthy milk."

The chemical and microscopic reports both show, that the milk of animals fed on distillery slop is far weaker than the Orange County, the proportions of their constituents varying, but in some being as one to three. In 1000 parts of Orange County milk, 35 were butter; while of 1000 parts of distillery milk, there were found but 15, 14, 13, and even 10 parts only of butter.

The original test of Mr. Reid, to discover the time necessary for coagulation, has evinced a most important fact, that the distillery milk will not coagulate in less than six hours, while pure milk, under the same influences, coagulates in one hour. This, with the observation of Dr. Clark, showing the peculiar tendency of the milk globules to conglomeration and the tenacity with which they adhere, appears to the committee a most important matter. This alone may serve to

account for the whole disturbance caused to the system of the child fed on this nutriment.

When the milk enters the human stomach its first change is coagulation, the second assimilation. If, therefore, a quantity of milk should refuse to coagulate, it remains as an indigestible substance in the stomach; and we should suppose would produce the effects usual to children when their stomachs are loaded with improper food,—produce convulsions, vomiting, and purging.

The deduction from the fourth head of Dr. Clark's report is perhaps as important, but less obvious. From the increased color of the epithelial cells, which may be ascribed to a diseased condition, and other peculiarities mentioned, we can suppose an unnatural state, perhaps caused by over-stimulation.

Such as we have mentioned are the condition of the animals and their appearance when dead, the *à priori* deductions, and the microscopic and chemical analyses. The question now arises, what are the real effects produced by the milk. The answer to this question is highly important. What is it?

About the year 1840, at the solicitation of R. M. Hartley, Esq., the following statement was signed by fifty-eight medical gentlemen of New York, including in the list many of the most distinguished in the profession, and among them, the president and vice-presidents of this Academy. "The undersigned, physicians of the city of New York, being requested to express our opinion in relation to the milk of cows fed chiefly on distillery slop, have no hesitation in stating that they believe such milk to be extremely detrimental to the health, especially of young children, as it not only contains too little nutriment for the purposes of food, but appears to possess unhealthy and injurious properties, owing in part, probably, to the confinement of the cows, and the bad air which they consequently have to breathe, as well as the unnatural and pernicious nature of the slop on which they are fed."

Prof. Charles A. Lee, formerly professor of Materia Medica in the University of New York, states the effects as follow:—"Children who are fed with 'still-slop-milk' have a pale, cachectic appearance, are extremely subject to scrofula, and

are liable to take every epidemic disease prevalent. To scarlet-fever, measles, hooping-cough, they are particularly subject, and will take them upon the slightest exposure; such children being apt to sink under any serious disease with which they may be attacked. There is a laxity of the solids and a vitiated condition of the fluids, which predispose them to disease in its most malignant form, &c."

Again he says, "I could give you any number of cases where the health of children has been utterly destroyed by the use of still-slop milk; and I could convince you that the *cholera infantum* itself, the great scourge of our city, is in fact chiefly caused by the use of this milk, either by the mother or child; for it is a singular fact, that in the large cities of Europe, where other causes of disease, with the exception of this, are as prevalent as in New York, this disease is absolutely unknown. Hence the efficacy of a removal to the country; as a change of diet is the necessary consequence."

Dr. Alex. H. Stevens, Prof. of the New York College of Physicians and Surgeons, states of a child about eighteen months old, who while a resident in his family was in perfect health, on moving to a hotel, was constantly affected with uncontrollable vomiting. The milk being suspected to be the cause, it was thenceforward sent from the Dr.'s family, when the complaint entirely ceased, and was renewed only with a renewal of the milk from the hotel, and this occurring so repeatedly as to leave no doubt of the cause.

Dr. W. N. Blakeman's child was afflicted with obstinate vomiting and purging, great loss of flesh, and extreme emaciation. Nothing gave relief. It was seen by many physicians, among them Dr. Dering, formerly Registrar of the New York College of Physicians and Surgeons, Dr. Beck, professor in the same institution, and many others. On discovering that the milk on which the child was fed, was from distillery slop-fed cows, it was changed, and the child was soon completely renovated, leaving no doubt in the minds of any of the physicians that the impure milk was the cause.

Dr. Cyrus Weeks had an infant child brought up by hand, fed for a single day on slop milk; a diarrhœa would commence,

which was speedily cured by omitting the food. This experiment was repeatedly tried, and with uniform results.

Dr. Trudeau gives the case of a child with protracted and severe diarrhœa, which was nourished upon milk alone. A change of diet entirely removed all bad symptoms in a few days' time. He states : "I am satisfied that distillery milk has done a deal of harm, and that the increased ratio of mortality among the children within a few years has no other cause."

These instances are, doubtless, similar to those in the experience of many. They seem to show conclusively, the deleterious qualities hidden in milk, produced in this unnatural manner.

Your committee, having expressed calmly and deliberately their opinions, do present for the consideration of the Academy the following resolutions.

Resolved, That, in the opinion of this Academy, the milk of cows shut up in stables and fed on distillery slops, is not only less nutritious than that of unconfined and well-fed animals, but is positively deleterious, especially to young children, and is a fruitful cause of many fatal diseases.

Resolved, That the Academy deems it proper to make known to the public authorities the existence of this evil, to the end that they may take such action in the premises as in their wisdom they may think fit.

All of which is respectfully submitted.

AUGUSTUS K. GARDNER, *Chairman.*

ON THE DIAGNOSIS OF

YELLOW AND BILIOUS FEVERS.

By Ashbel Smith, of Texas, M. A.,

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READ SEPT. 6, 1848.

Mr. President:

Gentlemen of the Academy of Medicine:

On Saturday and Sunday last, I visited the yellow fever district on Staten Island; one day in company with your distinguished and venerable president John W. Francis, M. D., the other day with the accomplished professor J. Henry Dickson, M. D., of the University of New York. We saw several cases of yellow fever in private practice as well as those in the Quarantine Hospital; we made two autopsies. I beg here to present my sincere thanks to the medical gentlemen attached to the hospital, and to those practising in the village, for their politeness and scientific zeal in facilitating our inquiries. The disease now prevailing on Staten Island presents to me the same symptoms and same pathology, without modification, such as I have often witnessed in the southern districts of our union. The yellow fever of Staten Island is identical with the yellow fever of our Mexican gulf coast. In the remarks I shall have the honor to present this evening, I shall limit myself exclusively to my personal experience in the diseases which form our subject. I have not consulted any author, nor referred to any book; by doing so I might have rendered my paper more complete; but the fellows of this Academy do

not want a compilation. If my remarks shall have any value, it will be because they are drawn from nature's book of facts.

Yellow fever is by many persons regarded as an aggravated form of bilious fever, or at least as allied to it by numerous and close affinities. I believe the two diseases to be distinguished by differences as essential, as permanent, as uniform, and as appreciable as the differences which distinguish any two febrile diseases whatever. I propose on the present occasion to state some of the points wherein the two fevers in question are unlike, and which authorize us to regard them as being of a different nature or character. Some nosologists, wholly unacquainted with yellow fever by observation, have classed it under typhus; this error, now generally abandoned by the medical profession, will be disproved incidentally in the course of my remarks, without the necessity of special refutation.

By yellow fever, I mean the disease which prevails in the seaports of the southern portion of our Union, more rarely in northern latitudes, and wherein the black vomit is a frequent symptom of cases which terminate fatally. By bilious fever, I mean that disease which prevails more or less throughout different sections of the United States, as well in towns as in country districts, is generally if not always of an intermittent or remittent character, and is commonly referred to malaria or miasmata for its origin. Entertaining the opinion that malaria or miasmata exhaled from the earth, are among the chief causes of this form of fever, I shall use the terms malarial, miasmatic, and bilious, indifferently, to signify the same disease: believing also that the malignant or congestive fever, whether remittent or intermittent, is only an aggravated type of the febrile affection now spoken of, I shall embrace it also under the same term.

Were I able to present before you a number of patients, some laboring under yellow fever and others with malarial fever of whatever type or grade, and especially if we could watch the progress of the several cases to their terminations in convalescence or death, I feel sure that I should not need afterwards to insist on the distinct natures of the two diseases. Much of the difficulty in diagnosis lies in our inability to con-

vey by language a distinct idea of the complexion or expression of the assemblage of symptoms, the physiognomy of any disease, to persons not practically acquainted with it. When we bear in mind that there is no known symptom admitted to be essential to and characteristic of fever in general, and when we further reflect that every symptom whatever has perhaps been met with in some cases of all the known forms of fever, the difficulty of my present undertaking fully appears. Nor shall I be disappointed should any one take each diagnostic symptom separately, and by a process of elimination seem to upset the position which I laid down in the commencement of this paper. By such a process all the distinctions of nosology may be set aside, all differences between diseases be shown to be fallacious, and disease be proved to be a unity. Only one thing would then remain to be done, the discovery of a "life pill" or "potion" for every ailment flesh is heir to. But I am addressing gentlemen of philosophical minds and practical sense.—In discriminating between diseases, we must take the assemblage and physiognomy of symptoms, and pathological ravages; the exceptional and accidental must be separated from that which is regular and uniform; and where symptoms deemed regular and uniform are wanting, we can often explain their absence by the predominance of other symptoms overriding or overwhelming them, if I may so speak, or by circumstances or treatment rendering the cases ataxic.

I shall trace the differences between bilious and yellow fevers, first by reference to some general laws which govern these diseases, and afterwards by a comparison of their symptoms and pathology.

Those persons who maintain the identity of yellow and miasmatic fevers, generally hold the former to be an aggravated type of the latter. If this opinion be correct, then it would seem that every case of yellow fever should be necessarily of great severity or malignity. This is not the fact; so far from it, that after the first period of a yellow fever epidemic, a large proportion of the cases, often more than half, are of a very mild character, sometimes only a febricula of a few or several hours' duration, or a brisk ephemera. It

would be scarcely accurate to say that these mild cases are only some general effects of the prevailing epidemic influences; for such mild attacks afford immunity against the disease during the same epidemic, and also in subsequent epidemics so far as my observation and information extend. Besides, these very slight or mild cases are in their physiognomy as distinctly characterized to the practitioner familiar with yellow fever, as are the most malignant cases. Perhaps my meaning will be more explicit by stating at length a parallel between yellow and bilious fevers. From the mildest intermittent, amounting only to a sensation of indisposition or *malaise*, recurring at regular intervals, to the most ferocious congestive fever, we trace a regular, uninterrupted gradation. And in my section of country it is common to see in the same individual, one grade converted into or succeeded by another within the period of a single interval; that is, a mild intermittent become congestive, or assume the form of a malignant remittent; and, *vice versâ*, the two latter forms disappear in the shape of a common ague and fever, or a mild remittent. So in yellow fever there is a regular gradation from the exceedingly mild febricula through all stages of severity to the most ferocious black vomit. We also see sudden conversions of grade in yellow fever similar to those mentioned as occurring in miasmatic fevers. The inexperienced practitioner is surprised to find the stomach deluged with black vomit in a case which some hours previously had given him no apprehension; and an invasion of excruciating severity may be succeeded by a state of extreme mildness, with scarcely a symptom of diseased action. To sum up, instead of malarial fever running into yellow fever, the same parallelism from great mildness to extreme severity, with sudden conversions not into each other, but into different grades of the same disease respectively, subsists in the two diseases.

I now proceed to another point. In places subject to epidemic yellow fever, sporadic cases do indeed sometimes occur in healthy seasons; but as a general fact, yellow fever is an epidemic. In Galveston, the disease prevailed epidemically in 1839, 1844, and 1847; nor have I ever known a case of domestic origin occurring in that city, except during seasons

of its epidemic existence. When miasmatic diseases exist, they are endemic and occur more or less every year, no year being wholly exempt from cases.

Bilious or miasmatic fevers pervade broad districts of country, and persons residing anywhere therein are subject to attacks of the same. Yellow fever is confined to the narrow limits of towns, which limits may be easily learned, and the disease avoided. In the State of Texas, throughout the level prairie country, intermittents and remittents are endemic. Few, indeed, are the plantations and farms, if any, that have not been the *habitat* of this endemic; most of them present cases every year. But I never heard it even suspected by any medical gentleman that a case of yellow-fever had originated anywhere in the interior out of the towns. If yellow fever were an aggravated or modified form of bilious fever, we should occasionally meet cases of the former in those sections of the interior country where bilious diseases in all their varieties prevail.

In my section of the Union, and I believe it is a general fact, villages and towns are more exempt from miasmatic disease, though not wholly so, than the surrounding country; on the contrary, yellow fever affecting concentrated populations, the farm-house and plantation are perfectly secure from its visits. Indeed, yellow fever is still more circumscribed; it very generally makes its appearance in a particular quarter of a town, in a district consisting of a single street, or a few streets, to which for the time being it is confined, thence extending more or less gradually over adjacent districts, and so regularly that its progress can be pretty accurately marked. No such rule obtains in regard to intermittents and remittents.

It has been alleged that, though miasmatic diseases prevail in regions exempt from the visitation of yellow fever, nevertheless the latter is developed only in a malarious atmosphere; and the conclusion is thence deduced, that common miasmata are essential to the production of yellow fever. What causes, if any, yellow and malarial fevers have in common, I am unable to decide. Nor am I sure that the following fact will be deemed relevant to this part of my

subject. Galveston is situated on an island, and has been thrice visited by yellow fever in ten years; yet residents in the city who do not visit the country or interior are, so far as my observation extends, quite exempt from agues, fevers, and all forms of miasmatic disease. There are apparently sources of miasmata within and adjacent to the city; perhaps the level site of the town and the regular trade winds of the warm season may prevent the collection and condensation of miasmata. It seems scarcely necessary to repeat, on the other hand, that yellow fever at times prevails epidemically in towns which are every year the *habitat* of endemic malarial fevers. In the city of Houston, bilious fevers are frequent and endemic. Yellow fever also has raged epidemically in that city equally as in Galveston, this disease having hitherto occurred in these two cities at the same periods. By comparing, then, the general hygienic state of Galveston and Houston, we have yellow fever epidemics and absence of miasmatic fevers in Galveston—and yellow fever epidemics with annual endemic miasmatic fever in Houston. It hence seems, that the two fevers in question are, for their origin and development, governed by laws, and produced by causes, irrespective and independent, to some extent at least, of each other. This position could be further corroborated by appealing to facts collected by other observers; but I limit myself to what has come within my personal observation.

In the yellow fever epidemics which I have witnessed, most of the first cases which occurred were malignant, and proved fatal. This fact is not noted as peculiar to epidemics of this disease; the same was true of the epidemic cholera which I observed in Paris in 1832. Whether the awful mortality in the first period of that epidenic is attributable to treatment, I shall not pretend to determine absolutely. In my opinion the disease was more malignant in its nature and rapid in its course in the earlier than during the later periods of its epidemic prevalence. So in scarlatine epidemics, I have remarked extreme malignity and rapid fatality of the early cases, with comparative mildness of those occurring in subsequent periods. Herein yellow fever is assimilated to other specific

epidemics; while no such character is impressed on miasmatic endemics.

Yellow fever requires as an essential condition for its first development a high range of the thermometer. Allow me to make my meaning clear. In a city where cases of yellow fever do not exist, the disease is not excited, the epidemic is not kindled, cases cannot originate unless beneath an ardent sun. According to my observation, the thermometer has marked at 3 P. M., upwards of 80°, more accurately 84° to 88° for several days in succession previously to the first appearance of yellow fever. When, however, the epidemic has been established, it continues to rage, though the thermometer fall permanently to a point at which the disease could not be originally developed. Now bilious fevers occur continually at temperatures too low for the origination of yellow fever, as well as during the extremest heats of summer and autumn. At first blush one might be led to think that the supervention of an ardent sun to the causes of bilious fever would convert this latter disease into yellow fever. But this cause alone is insufficient, for then we should find such conversions taking place in the country districts; which, as we have seen, never happens.

It may be noted among the general points of difference between the diseases in question, that it was once commonly held, and not a few still entertain the opinion, that yellow fever is contagious and importable, while no suspicion of a similar quality attaches to bilious diseases.

I have made incidental allusion to causes or rather conditions necessary for the production of yellow fever, without attempting to define them. We are fully authorized to declare that several causes or conditions must co-exist; the present state of our knowledge does not authorize us to say that some of these causes may not be common to bilious and yellow fevers.

Having thus set forth some general points concerning yellow and bilious fevers, I now proceed to consider those symptoms which strongly and definitely distinguish the two diseases from each other. It would seem at first blush that my purpose would be best accomplished by grouping together

the symptoms of these diseases respectively; but as they are well known to all of you, I proceed at once to note their peculiar and discriminating symptoms.

Bilious fever is intermittent, remittent, or continued. The continued type cannot in strictness be so called; for there are marked exacerbations or remissions every day or every other day.* Yellow fever is neither intermittent, remittent, nor continued; there are never two similar recurring paroxysms or exacerbations. It is essentially and uniformly a disease or fever of one single paroxysm. This paroxysm consists of states or stages, as follows: *first*, of depression; *second*, of vascular excitement, which subsides of itself, if not rendered ataxic by injudicious medication, into a state of apyrexia, terminating at once in convalescence or in the *third* stage of prostration with new and peculiar symptoms; this last is the stage of hæmorrhage or black vomit. The state of vascular excitement is never renewed or repeated any more than is the eruptive fever in the succeeding stages of small pox. I have indeed seen cases when the subsidence of the vascular excitement was prevented, and the disease converted into a fierce continued *causus*, *obviously* the effect of injudicious medication. Frequently also, there is a feverish state continuing to the close of the disease, but it bears no likeness to the stage of vascular excitement alluded to. On the contrary, if an artificial febricula is not kept up by medication, the patient falls into a condition the most opposite to pyrectic. In many hundreds of cases of yellow fever which have fallen under my observation, I have never witnessed a second febrile paroxysm in the same case, like the first. I have indeed seen yellow fever supervene on an intermittent in persons affected with the latter coming into a district where the former prevailed; but in these cases the yellow fever occupied at once the whole ground, if I may so speak, and marched its course

* I cannot recall any case of continued bilious fever with *quartan* exacerbations. The periodical character of this fever sometimes nearly disappears in cases which assume a nervous form towards their termination. But in all such cases which have fallen under my observation, the fever has been irritative, and dependent on some anatomical lesion generally of the mucous coat of the alimentary canal; the lesion being a sequela of the previous periodic fever.

to its termination, regardless of the paroxysmal disposition of the disease which it had thrust out. In like manner I have seen the malignant miasmatic or congestive fever seize a person convalescent from yellow fever, who had been previously exposed to concentrated miasmata in the country districts; but the march of the two diseases was distinct, nor could they be confounded. Yellow fever, as has been stated, is sometimes rendered ataxic by injudicious treatment or neglect; but a second paroxysm similar to the first is not reproduced. To sum up: *bilious fever consists of similar paroxysms or similar exacerbations; yellow fever never repeats itself, is not paroxysmal, has no character of periodicity.*

Relapses and second attacks are frequent in malarial fevers, and the liability of the system to these forms of fever is increased by previous attacks. *Not unfrequently it is impossible for a patient to become firmly convalescent but by removal from the malarious district.* Herein are to be noted striking differences between the two diseases whose diagnosis I have undertaken to set forth. *Relapses*, properly so called, do not occur at all in yellow fever, and second attacks are very rare, and especially in the same locality; and *the immunity may be regarded as absolutely complete, provided the patient remains permanently and continually in the epidemic district.* In this disease, however, patients imperfectly or seemingly convalescent do by imprudence in eating or exposure fall into the last stage or that of prostration, characterized by black vomit, hæmorrhages, etc. There is a deceitful pause between the subsidence of the vascular excitement and the setting in of the period of prostration which may last even for several days; during this time the patient may eat, walk about, and be apparently convalescent, when black vomit may suddenly supervene and the case terminate fatally. In such cases, however, the vascular excitement, or in one word the whole paroxysm, is not renewed as in periodic fevers, but the fatal symptoms supervene without prelude upon the delusive pause. *The hæmorrhagic state of the system subsists during all the time of the delusive pause*, is easily cognisable, and distinguishable from established convalescence by practitioners familiar with the disease.

As already stated, second attacks of yellow fever are rare, and when they do occur, it is only on exposure in *different* epidemics. I have seen and treated several hundreds of cases without a single relapse, and at most but one ascertained case of second attack.

This is not the place to enter upon the consideration of how far and under what circumstances an attack of yellow fever in one section of country furnishes immunity against the disease on exposure afterwards in other *habits* of this epidemic.

One attack of yellow fever protects a person against a subsequent one, as we have seen; but yellow and bilious fevers afford no *reciprocal* immunity against each other. I am here led to remark that persons born and reared in southern malarious districts, who have never visited a cold climate, are as liable to yellow fever on entering an infected district as any other persons whatever. A pretty numerous family, all born and reared in the vicinity of Mobile, but who had never been exposed to yellow fever in that city or elsewhere, removed to Galveston. They all had attacks during our last epidemic, two of which proved fatal. The disease was as violent as in persons coming from the highest northern latitudes.

Black vomit is the natural and regular termination of fatal cases of yellow fever, when the disease has been allowed to run its course unchecked or unmodified by treatment. I have been called to see a considerable number of persons in the last stage of this disease who had previously taken no medicine whatever. In every instance I found them pouring forth black vomit. And in general, an examination after death of those who ejected black vomit during life, almost always reveals in the stomach this matter, where death took place within ten days after the invasion of the disease.

Several authors mention black vomit as being of not very uncommon occurrence in other diseases than yellow fever. This does not correspond with my own experience. I have indeed many times seen a brownish saburral matter vomited in malarial fevers, but it would be a great error to confound this with black vomit, from which it is easily discriminated. Matters have been twice brought to me, which it was very difficult if not impossible to distinguish from genuine black

vomit, ejected from the stomachs of patients laboring under other disease than epidemic yellow fever. But I am bound to declare that during a most extensive practice in the malignant and congestive bilious fevers of the southern country, I never saw genuine black vomit, in my own practice, out of the pale of well characterized yellow fever. Of the extent of my observations in malignant bilious fevers, some notion may be formed from the fact that in 1837 I prescribed for 119 cases of this disease in one day; most of them being soldiers of the old Texas army. So extensive a practice ought to have furnished at least one case of black vomit, if it is of frequent occurrence in paludal fevers.

Black vomit is so peculiar and distinctive a symptom of yellow fever that I have chosen to consider it separately; it may nevertheless be perhaps regarded as part of or resulting from a general hæmorrhagic disposition which exists in this disease; which general *hæmorrhagic disposition* or character is eminently diagnostic between bilious and yellow fevers. Hæmorrhages are rare, quite rare in bilious fevers; they are *extremely common* in yellow fever, much more common than is usually supposed; often occurring unsuspected either by patient or physician. My attention has been much directed to this characteristic hæmorrhage. Autopsies have revealed to me numerous and copious internal hæmorrhages whose existence was wholly unsuspected during life. I have found them everywhere in the abdominal cavity; in the large and small intestines and stomach; beneath the peritoneum; in the cellular substance investing the kidneys and renal capsules; between the laminæ of the sac of Willis; into the kidneys; into the substance of the liver; into the bladder of urine; from the uterus; from the surface of the scrotum; from the gums, nose, and ears; into the subcutaneous cellular substance of nearly every portion of the surface. The internal hæmorrhages alluded to as being revealed only on dissection are not of a few drops or a teaspoonful or two; I found them in quantities of a gill, half pint, pint, and quart. I have stated that these hæmorrhages are often unsuspected; they occur, moreover, in cases which though severe present no immediately alarming symptom. I have drawn off bloody urine

where the patient did not complain of the slightest uneasiness; I have learned, on inquiring, the existence of free uterine hæmorrhage which the patient had not mentioned, supposing it to be a return of her catamenia. On examination in cases not regarded as dangerous, I have found extensive infiltrations of blood into the cellular substance of the lower extremities, giving them a marbled appearance, which, nevertheless, had escaped the notice of the patients. Hæmorrhage from the gums is common where not a grain of mercury has been administered. Now, hæmorrhage from any part of the system is extremely rare in miasmatic diseases; I do not remember at this time a case of hæmorrhage from the gums in paludal fevers without mercurial salivation or a strongly marked scorbutic diathesis.

In illustration of the frequency of hæmorrhage in yellow fever, I will state that at my recent visits to Staten Island, among ten patients I saw in the hospital and village, there were three cases of hæmorrhage from the gums; one from the bowels; one of ulcerated scrotum; five of black vomit. It is proper to state that some of the patients had bleeding from different parts of the system at the same time.

The peculiar, and not easily describable physiognomy of yellow fever is so constant that it ought not to be omitted. To one familiar with the disease, it is as striking and unmistakable as are the ways and physiognomy of a drunken man.

There are several symptoms, which, though not of themselves characteristic of yellow fever, should nevertheless be mentioned in contrasting this disease with malarial fever, from their frequent occurrence in the former, and their infrequency in the latter disease.

In a majority of fatal cases of yellow fever, the pulse in the last stage is preternaturally slow, unless quickened by stimulants, and sometimes in despite of stimulants. On merely looking at the patient in the Quarantine Hospital, I remarked immediately to Dr. Whiting, that man must have a slow pulse, it cannot be over fifty. Being counted, it was thirty-two—it fell in a few hours to eighteen, several hours before his death. An intervenient feeble pulsation was sought for

in vain. In bilious fevers it is rare to find the pulse below the healthy standard of frequency, except after the complete abatement of the disease.

In yellow fever, the skin, after the subsidence of the vascular excitement of the setting in of the third stage, is dry, has a peculiar harsh feel, and its temperature sinks below the healthy standard; when cerebral congestion co-exists, there is moisture about the wrists and ancles. In advanced stages of bilious fever, or in severe congestion, if the surface is cool, the extremities are bedewed with sweat, more or less clammy and copious.

In severe cases of yellow fever, the urine is generally of an intense saffron yellow, not unfrequently deposits blood on standing, as heretofore stated, and the bladder is often insensible to distension. In bilious fever the urine is more commonly of a reddish hue, and scanty, or copious and limpid; nor have I remarked insensibility of the bladder to its presence.

There is an occasional symptom in yellow fever which I have not met with in any other febrile affection; it is spontaneous and genuine venereal appetite in the stage of extreme prostration, and indeed of irremediable danger. Memoriâ teneo meipsum hoc morbo graviter laborantem, momento quidem periculosissimo, ignibus veneris insolitis vehementer arsisse.

Great serenity of mind, or rather sluggish indifference, marks the final fatal stage of yellow fever; this, though not peculiar to the disease in question, is ~~not~~ quite observable as contradistinguished from the usual condition of the mental faculties of persons verging to death in bilious fevers.

Among the less characteristic symptoms deserving remark, is the frequency of attacks in the night by yellow fever, and the comparative infrequency of nocturnal attacks of paludal fevers.

Yellowness commencing commonly before death, and becoming more intense after that event, is so frequent as to have given name to the disease; yellow suffusion is, nevertheless, occasionally met with in bilious fevers; but my observations lead me to think that a very noticeable increase in

intensity after death is of rare occurrence in the latter disease.

I now proceed to note briefly the pathological distinctions between the two diseases under consideration. These are in yellow fever, the general hæmorrhagic disposition, black vomit, yellow or fawn colored liver, and a pathological condition of the mucous membrane of the stomach. Much that I should have to say on the hæmorrhages and black vomit has been anticipated under symptomatology, and need not be here repeated. From the frequency of black vomit in fatal cases the Spaniards have thence called this disease *vomito prieto*, or *negro*. This matter is so seldom found to be wanting in the stomach in such cases, that though some may have been ejected during life, I have met with it in all the autopsies I have made, with perhaps a single exception. This exception was in the autopsy of an individual whom I never saw during life, but who died, I have reason to believe, of yellow fever. In a very few cases which were fatal from the twelfth to the twentieth day after attack, I suspected the stomach would be found to contain no black vomit, but opportunity was not afforded me to ascertain the fact by dissection. So, also, in the body not examined of a person whose case was rendered ataxic by treatment, and terminated fatally on the eighth day, I did not suspect the presence of black vomit in the stomach. I note these conjectural opinions in order to qualify the general statement of the results of my autopsies. I have never met with black vomit in my dissections in paludal fevers.

I have always found the gastric mucous coat in a pathological condition in yellow fever. In the formation of black vomit, portions of the gastric mucous membrane are congested or gorged with blood, which is poured into the cavity of the stomach possibly by simple effusion, or, as I believe, by an imperfect or diseased vital process. The portions of the coat which furnished the black vomit are softened and generally thickened; but in the present state of pathological science and language, these effects cannot be distinguished from the effects of other diseases upon the same tissue. They will not, therefore, serve the purpose of practical diagnosis.

I have, however, had opportunity in several cases to observe portions of the mucous coat in the state of sanguineous engorgement previous to the formation of black vomit—other portions of the tissue having furnished this fluid and being in the condition above mentioned—and I do not hesitate to say that I can distinguish the sanguineous engorgement in this disease from the congestion in any other disease I have yet witnessed. Common gastritis can occupy one portion of the mucous coat, while another portion of the same furnishes black vomit. I shall not here pursue this point further, as my present object is practical diagnosis.

The liver in yellow fever is paler than natural, varying from a pale straw to a bright coffee and milk color; the hue is uniform or mottled; in a few rare cases it is of a deep dark color. The intensely dark liver, of which I have seen two cases, was owing to a hæmorrhage into its parenchyma. In all my other dissections, the fawn color of Louis has been distinctly noted, with the exception of one liver which was not remarked on. Three autopsies were made at the Quarantine Hospital on Sunday last; two of yellow fever cases, and one of dysentery. The livers of the first two were of bright coffee and milk hue, and their stomachs contained black vomit; these livers contrasted strongly with the mahogany colored liver of the dysenteric body which was emaciated and exsanguious. I beg here to state that this yellow discoloration was noted by me in six out of eight yellow fever autopsies *published before* the publication of M. Louis's book, and before I had heard of his observations. But to him alone belongs, so far as I know, the merit of calling the special attention of medical men to this pathological appearance.

I have not seen black vomit in the stomach nor the yellowish discoloration of the liver, nor the internal hæmorrhages in my dissections of bodies dead of bilious diseases. It does not seem necessary for our present purpose to describe minutely the pathological appearances in bilious fevers. It is possible that in dissections made years ago of fatal cases of bilious fevers, I may have noted as natural, livers which more careful observation should have described as bronze colored. In the autopsy of a case (Capt. Thompson), of malignant

congestive fever coming from Tampico, last year, the bronze colored liver as described by Drs. Stewardson, Swett, and others, was very strongly marked.

I will here state that the present year, I have seen a *distinctly marked straw colored liver* in a child that died of cholera infantum with metastasis to the brain.

I have not deemed it necessary to establish a formal diagnosis between yellow and typhoid fevers. The fact that yellow fever requires an ardent sun for its development, while the typhoid affection prevails most in colder temperatures, as well as the universally known symptomatology of the two diseases, sufficiently distinguish them from each other. Still it may be proper to add under the head of pathology, that in my numerous dissections in yellow fever, I never met with the anatomical lesion of typhoid fever, to wit, ulceration of the elliptic patches of Peyer. Typhoid fever, though extremely rare in the coast district of Texas, is nevertheless sometimes met with. I was invited last August by a medical gentleman to Galveston to see a lad seven years of age, a native of Texas, and always resident there, laboring under fever. The case presented unmistakable symptoms of the typhoid affection; he died the 15th day from the attack; the autopsy made by me exhibited numerous and large ulcerations of Peyer's glands. For many days preceding, the weather had been dry and warm, the thermometer at 3 P. M, ranging from 84° to 90°; the barometer at 10 A. M., from 30.16 to 30.20 inches.

I have thus nearly completed, how imperfectly I am very sensible, the task I imposed on myself of setting forth the distinctive characteristics of bilious and yellow fevers. My main difficulty, as already intimated, has lain in my inability to paint adequately the physiognomy of the assemblage of symptoms which constitute the two diseases respectively. To my mind's eye, they neither blend with nor run into each other; but the prominent features of demarcation can be clearly and fully appreciated only by personal observation. A person must see with his own eyes in an *intermittent*—the chill, the fever, and complete intermission—to be succeeded the following day or some subsequent day, at regular intervals,

by a similar chill, fever, and intermission, and this state to continue for days, months, and even years. In a *remittent*, he will see chilliness, exacerbation of the fever, great remission—and these symptoms repeated with great regularity, with diurnal or double tertian regularly for a number of days. If the bilious fever be of a *continued* type, he will witness exacerbations and remissions occurring with the regularity of remittent, and like it for days in succession. If he encounters a malignant intermittent or remittent, usually called *congestive* fever, the patient is often comatose or delirious, his extremities and part of his body are dank and colder than a corpse, in an atmosphere perhaps of from 80° to 95°. With this deadly coldness of the surface there is a burning heat of the entrails, shown by unquenchable thirst and incessant cries of the sick man for cold drinks, unless the stupor be so complete as to mask all sensation. In an hour or two, or a few hours, the scene is changed, and if reaction be complete, a burning heat of the surface has succeeded to the algid dankness. This state in its turn gives place in a few hours to an intermission or remission, as in the milder forms of bilious disease, with perhaps scarcely a symptom of urgent danger. But on the following day, or a subsequent one, a paroxysm similar to the first is repeated. After a few repetitions, if the disease be not arrested by art, or converted into a milder form, it terminates fatally in the cold stage.

If a case of *typhoid fever* be presented, he sees a febrile disease coming on slowly and stealthily, requiring days to reach its acme, and days for its subsidence. Changes which take place in hours in bilious or periodical fevers, require as many days in typhoid fever. Exacerbations are wanting or scarcely distinguishable; nervous and other symptoms rare in other diseases are common in this; convalescence is slow; tardiness is impressed on all its stages. If fatal, ulcerations in the lower part of the small intestines are found on examination after death.

In yellow fever, in a case not rendered ataxic, one sees the depression or chill, the fever with the agonizing pains of the forehead and loins; these subside spontaneously within three or four days into a state of apyrexia. This is the only

paroxysm; there will be no subsequent vascular excitement. The patient may now become convalescent without further adverse symptom; but he is very liable to sink into a state of mental sluggishness, he kicks off the bed-clothes, sits up on the edge of the bed, or walks about the chamber with a peculiar unconscious indifference; suddenly a turbid dark fluid gushes from his mouth, or the black vomit makes its approach more insidiously, first an occasional slight eructation of air, then a hiccup with the ejection of a teaspoonful or two of a thin whey-looking fluid; the fluid is ejected more frequently, in larger quantities, and darker and more turbid; hæmorrhages have taken place from his gums, or other parts of his system; his pulse become slow and sluggish; his respiration infrequent and protracted, becoming more infrequent, protracted, and feeble, so that you can scarcely tell when he breathes his last; or he dies suddenly with a copious gush of black vomit; or he drops dead while walking about. An examination after death reveals the black vomit, the hæmorrhages, the ravages in the stomach and liver already noted.

I have spoken of yellow fever as a disease of pretty uniform character. Such has been my observation; and the descriptions of those practitioners whose treatment has seemed to me most judicious and least perturbating, present a similar and pretty uniform symptomatology; quite as much so as any disease with which I am acquainted. Cases have been rendered ataxic by injudicious treatment, and nosological varieties have been thus multiplied at the expense of true science—and of patients too. In the commencement of my practice, I made bad ataxic cases. Such cases do, indeed, occur without any fault of the practitioner, or exposure of the patient; sometimes they are produced by the overwhelming violence of some symptoms, marking or ruling out others. Slight modifications are, it is true, presented in different epidemics and in different periods of the same epidemic, but it is chiefly in the minor symptoms, or in the comparative severity of the leading symptoms; so that I regard the formal division of yellow fever into different

species, types, and forms, as being unphilosophical and attended with no practical advantages.

Gentlemen, Fellows of the Academy of Medicine, such are the leading differences between the laws, symptomatology, and pathology of bilious and yellow fevers, as they have been presented to my observation; if I have failed to establish clearly a differential diagnosis, I beg you will attribute it to my inability to seize and describe adequately those features in the two diseases which exist in nature, and distinguish them from each other by specific and permanent characters.

ESSAY ON THE
VALUE OF THE SETON,
AS A REMEDY IN UNUNITED FRACTURES,
ILLUSTRATED BY CASES.

By Valentine Mott, M. D.

The surgery of the present day is pre-eminently utilitarian in its character; and the efforts of its most successful cultivators are marked by a desire to establish, by accurate philosophical deduction from the well ascertained facts which constitute our art, the true and real value of our therapeutic resources and operative procedures, rather than by the introduction of novel operations of equivocal value. But a few years ago, every new number of a medical journal introduced to our notice some innovation in mechanical and operative surgery; surgeons seemed determined to secure immortality by novelty, and our manuals of operative medicine have become clogged and unwieldy by the accumulation of various novel processes which, for the most part, lack in utility what they claim in originality.

Time, which determines the value of men as well as of their inventions, has given us a mass of experience, in the way of well ascertained and authentically recorded facts, which, if closely studied and thoroughly elaborated, will serve as the only firm and certain foundation on which to build the advancement of our science. "We know too well," remarks a recent writer, "how enormous is the quantity of facts which have been accumulated in every department of medicine during the last two thousand years; the most pressing want which our science at present suffers, is the reduction of this vast mass into separate groups, and the establishment of cer-

tain defined principles. * * * It is probably only by the assistance of the statistical test that we shall finally arrive at fixed conclusions respecting the efficacy of various modes of treating a disease, and the prognosis to be formed respecting the issue of various diseases."—(Bisset Hawkins in Cyc. Prac. Med.)

The leaders in the various departments of medical science have already adopted this mode of study, and results have been obtained of which we all enjoy the benefit. In the department of pure medicine, the researches of Louis on Phthisis, of Boulliaud on Pneunomia, and of Andral and Chomel on Typhoid Fever, have placed them at the head of the philosophical investigators of that branch of the healing art. The statistics of Collins, of Lee and Boivin, have added much to the certainty of obstetrical science; and, in our own department of surgery, the celebrated tables of Malgaigne on Hernia, and the enlightened labors of Norris, of Philadelphia, on the Statistics of Amputations,—of the ligature of large arteries, and of the results of the treatment of ununited fractures, have secured for them an enviable celebrity throughout the scientific world.

The sum of each individual's experience in the treatment of disease constitutes the basis of his judgment, and stamps the value of his individual opinion, in a given case. The truth of this proposition will be admitted on a close examination; for no individual is capable of accurately appreciating the recorded experience of others, except by measuring and comparing it with his own personal knowledge—which, in the end, gives all its practical value to medical learning. As the personal experience, then, of each individual—the most valuable portion of his knowledge—for the most part must die with its possessor, it follows that our science consists alone of those facts which have been placed upon record, by individuals, to serve as the basis of inductive reasoning, and of statistical analysis. These facts are valuable in proportion to their ascertained authenticity, and the character for accuracy of the observer. It is the duty then of every medical man who is anxious for the legitimate advancement of his science, to do his part, however humble, by placing on record such facts as may have

fallen beneath his notice, for we may rest assured, that however trivial they may be, if accurately observed, posterity will acknowledge their value. They will take their appropriate place in those statistical results on which in future time medicine will rest its claims to take rank as one of the exact sciences.

I have made the foregoing remarks by way of apology for laying before the Academy the result of my experience on a subject which has recently undergone statistical investigation, with the view of placing on record, should the Academy deem them worthy of a place in its archives, some cases which have never been heretofore published.

I refer to the use of the Seton, as a remedy for Ununited Fractures, an operation which the world owes to an American surgeon—the illustrious Physick, of Philadelphia; and which the researches of another American surgeon, Dr. Norris, have statistically proved to be, in the present condition of surgical science, the most certain remedy that we possess for that unfortunate occurrence.

It is worthy of remark that the elaborate and valuable paper of Dr. Norris forms the basis of the article on "Ununited Fracture," in Nos. XI. and XII. of the *Cyclopædia of Prac. Surgery*, which, completed by T. W. King, Esq., constitutes the latest monograph on this subject, which we have from an English authority. The conclusions arrived at in this article, with regard to the comparative value of our remedial means, drawn from the tabular view of one hundred and fifty cases, compiled by Dr. Norris, are embraced in the following quotation:—

"Of forty-six cases in which the *seton* was employed, thirty-six were cured, three partial cures, five no benefit, two died.

"Of thirty-eight cases in which *resection* was employed, twenty-four were cured, one partial cure, seven no benefit, six died.

"Of thirty-six cases in which *pressure* and rest were employed, twenty-nine were cured, one partial cure, six no benefit.

"Of eight cases in which *caustic* was employed, six were cured, two received no benefit.

"Of eleven cases in which *frictions* were employed, eleven were cured.

"Of eleven cases in which other methods were employed, viz. *iodine, injections, hot iron and amputation,* seven were cured, one received no benefit, two died, and one remains uncertain."

Under the head of seton are embraced its varieties, viz. the conical seton of Weinhold, the silver wire of Sommé, the double seton of Oppenheim, and the variation adopted by Seerig. Under the head of resection are included all cases in which the ends of the bones were scraped, rasped, or excised.

The opinion expressed by Nelaton, perhaps the most recent surgical authority amongst the French, in his incomplete work on Surgical Pathology, will give an idea of the appreciation of the various remedies in use by the surgeons of France. After enumerating the resources mentioned above, he states that "in the present condition of science it is impossible to determine which of these remedial means would deserve our preference in a given case," and proceeds to quote the following opinion of W. A. Bérard: "When a fracture is found to remain ununited after the lapse of the usual period, reapply the apparatus, give it all the additional solidity possible, and renew it at long intervals; seek, meanwhile, for any constitutional cause in your patient which could prevent union, and remove it if possible. If these means are insufficient, make use of blisters and other irritating applications to the limb over the seat of the fracture; if these fail, resort to friction of the fragments, and replace the apparatus. If you are still unsuccessful examine carefully if your case present any indications (such as caries, necrosis, &c., at the seat of the fracture) which would render resection preferable to the employment of the seton. Should this indication fail, pass a seton between the fragments, and if necessary saturate it with irritating substances; apply caustic to the fragments, and if there is still no consolidation, resection remains to be tried."—Vol. i. p. 684. Velpeau concludes that the seton "is a very uncertain means, and one for which I should prefer to substitute exsection, where frictions and the starch bandage had not answered,

and it is one moreover which is not always unattended with danger. M. Weinhold, in applying it to the neck of the femur, brought on caries and suppuration in the cotyloid cavity and pelvis, which ended in the death of his patient."—(Am. ed., vol. ii. pp. 689, 690.) If M. Velpeau was influenced in his opinion as to the danger attending the use of the seton by the case of M. Weinhold, which he quotes, we fear he has been led into error. In the only account of this case to which we have access, it is distinctly stated that Prof. W. "was unwilling to try any operation, as the symptoms clearly indicated chronic inflammation, if not actual suppuration of the hip joint. The patient, however, had heard of his cures, and was determined to take the chance. He died of hectic at the end of six weeks."—(*Ed. Med. and Surg. Jour.*, vol. xxvii. p. 399.) Surely this case should not detract from the merits of the operation; it is the only unsuccessful case reported in the essay of Professor Weinhold, in which four successful cases are related, three of which involved the thigh-bone. Professor Gibson, of Philadelphia, states that he has operated in ten or twelve cases with the seton, and has uniformly found it more or less successful.—(*Surgery*, 1841, vol i. p. 336.) These cases are not contained in Dr. Norris's table; it is to be regretted that they are not recorded in a more tangible form. Dr. Physick is said, by Prof. G., to have twice tried the seton in the thigh; and to have expressed himself as fearful that it would not succeed in any case of the kind.

An interesting fact is mentioned by Prof. Colles of Dublin, in his lectures, from which it would seem that we have a therapeutic resource in these cases which is not generally recognised by authors. "An army surgeon told me," said he, "that while quartered in the West Indies, he once had five or six cases of ununited fractures together, and they were long enough under his care to have been, in the ordinary course of things, united three times over; these men, with their regiment, embarked for Europe; on their arrival the surgeon inspected all the men, and on examining these men who had fractures so long without union, he found all their fractures united, without the patients or himself being at all conscious of when the union had taken place, as their attention had not

been aroused by the occurrence of pain, or peculiar feeling of any kind."

Electricity is said to have been used with good effect in producing consolidation in some cases; I have made use of it on several occasions without benefit.

I shall now proceed to detail some cases in which I have used the seton:

Case I. In 1817, after having employed in vain, pressure combined with rest, blisters, and electricity, in an ununited fracture of the tibia, I passed a seton between the ends of the bones. At the end of three months the cure was perfect.

This case is reported in the Med. and Surg. Register of the New York Hospital, Pt. II., v. i. p. 375.

Case II. In 1819, after employing the means enumerated in Case I., I made use of the seton in a fracture of the femur, which had failed to unite. It was allowed to remain three months. At the end of a year the bone was found firmly united.

This case is also recorded in the Hospital Register, as well as a third, of the humerus, in which the result of the employment of the seton was not certainly ascertained. I have reason to believe, however, that this case was also cured.

Case IV. A. B., of Florida, thirty-five years of age, of a good constitution, and in the enjoyment of excellent health, in 1822, received an injury in which he fractured the tibia and fibula of one of his legs; in both the bones the fracture was simple. He was treated with the limb in an extended position, and during the treatment no unusual circumstance occurred. At the end of six weeks, on examining the limb to see if the bones were in a state of consolidation sufficient to allow the splint to be removed, it was found that the fibula was firmly united, but that the tibia was as movable as at an early period of the injury.

He was directed to remain quiet a few weeks longer, to give an opportunity for union to take place. This not being accomplished, he came to New York, after the lapse of seven months from the receipt of the injury, and placed himself under my care. At this time his general health was good; he had attempted to use the leg for some weeks, by means of

crutches, and bearing a slight weight upon it gave him but little pain; the bone yielded, however, at the fractured point by the least pressure upon the foot.

A strip of blister plaster an inch and a half in width was applied around the limb, at the seat of the fracture, several different times. Shocks of electricity were from time to time resorted to. The ends of the bones were repeatedly rubbed together briskly; and he was directed to add to the irritation about the point of fracture by more active use of the limb.

These means were faithfully tried for several weeks, but without the least appearance of further consolidation of the fracture. The seton was now proposed to him, as the means which promised best success; he readily consented, and it was accordingly introduced. An incision was made on each side of the limb, and on repeated trials it was found impossible to make a pointed stilet penetrate the ligamentous attachment of the fragments; an ordinary gimlet was then employed, and, through the hole thus made, an eyed probe was passed, containing a skein of silk. The limb was then placed in carved splints, and kept extended on the heel; sometimes it was shifted on the outside, and flexed.

Very little pain or inconvenience attended or followed the operation. He was allowed to continue his ordinary full diet without interruption.

After the lapse of six weeks the bones began to be firm, and from this time I began to remove the seton, thread by thread, until it was all taken away—by which time the bones were perfectly consolidated. The whole time occupied in the treatment, from the introduction of the seton until the union of the bones, was about two months and a half.

Case V. George Westerfield, a boy aged twelve years, was sent on to me from Ohio, in 1826, with an ununited fracture of the right *os brachii* of eight months' standing. His general health was good, and no cause could be assigned for the want of consolidation of the fracture, except the difficulty of keeping the parts in apposition. The seat of the injury was about the middle of the bone, and the fractured ends were conical, and separated from each other at least an inch; no pain was

experienced on rubbing them together, and there was free motion in all directions. With the ordinary seton needle I passed a seton through the arm, as nearly as possible between the separated ends of the bones. The arm was afterwards encased in splints, and kept as quiet as possible. After the more active inflammation had passed away, he was allowed to carry it in a sling, properly secured and protected. Weeks, and even months passed away without any evidence of union at the fractured ends. At the end of six months, as there was not the slightest prospect of amendment, I withdrew the seton, and allowed the sinuses to heal. After waiting for some weeks, with no evidence of change, I determined to try a second seton; this was again introduced, and of larger size, and after continuing it for several months without benefit, it was also removed.

I now put him into the hospital for further treatment; and after stating, in consultation with my colleagues, the history of the case and the practice that had been pursued, it was determined, as the only alternative of amputation, to try exsection of the ends of the fragments. This was accordingly done by Dr. J. Kearney Rodgers, who followed me in my tour of attendance. In addition to sawing off the ends of the fractured bones, Dr. K. passed a silver wire through each end of the sawed bone, and after twisting the ends together, brought them out of the wound through a canula. The wire from one end of the bone wore out in a few days, and it was removed from the other a short time after. No great degree of inflammation followed the operation, and in a few weeks, as the wound healed, the os brachii became firmly consolidated.

This is the first time, as far as our knowledge extends, that the ends of the bones have been connected by the wire suture after the operation of exsection. As will appear in the next case I have successfully adopted the same practice, and it has been also tried in numerous cases by New York surgeons; I am not aware that there is a single case on record in which it has failed.

Since the introduction of this operation into the surgical practice of our city, M. Flaubert, of the Hotel-Dieu of Rouen,

in France, has adopted the same method with perfect success, and M. Malgaigne in his Operative Surgery has given to him the credit of originality in this operative process. He may be correct with regard to the originality, but we know that we can claim the priority of years, for the surgery of our own country.

Case VI. James Norton, a middle-aged man, of good constitution, was admitted to the N. Y. Hospital in April, 1830, with an ununited fracture of the humerus above its middle, of three months' standing. The ends of the fractured bone were widely separated, and could not be approximated. A seton was passed through the limb on the 10th of April, and allowed to remain until the 28th of September without producing the slightest benefit, when it was withdrawn. Resection was then practised, and the fragments retained in contact by the suture of well annealed iron wire. Two or three weeks after the operation the wire came away, the arm became gradually stronger, and on the 15th of December, when he was discharged, the union was complete.

I attribute the want of success which followed the use of the seton in this case, to the separation of the fragments. It is reported by Dr. A. C. Post, in the New York Med. Jour., i. p. 275.

Case VII. L. M., aged fifty-five years, of active habits, although rather corpulent and phlegmatic, fractured his right femur at its upper third, in January, 1833. After the lapse of ten days, at which period the inflammation had subsided, his limb was placed in the double inclined plane. No unusual circumstance occurred during the ensuing five weeks; the patient was very tractable and obedient, evincing the most earnest desire to get well. On examining his limb, however, at the commencement of the seventh week, to my surprise, and the great disappointment of my patient, it was found that no union had taken place at the seat of fracture. He was requested to remain quiet for two or three weeks longer, to which he cheerfully consented. At the end of this time I found the bone still ununited. The limb was now removed from the double inclined plane, and four thigh splints substituted, with pillows for a support. In this position it was kept

for about ten days; each day the dressings were removed and the ends of the bones briskly rubbed together; friction with camphorated spirits was afterwards applied to the thigh and leg, and the bandages and splints reapplied.

After this, the limb was restored to the inclined plane, and the patient requested to remain in bed, and use generous diet. This course was steadily pursued for six weeks, at the end of which time I found upon examination that no union had taken place.

Believing that no benefit would arise from any further delay, I now proposed the introduction of a seton, to which he readily consented. Some considerable difficulty attended the operation, from the lower fragment of the femur being drawn, as usual, behind the upper. To securely lodge the seton between the fractured ends, I was obliged again to resort to a large spike gimlet in the form of a *bit*, to make a passage for the eyed probe, as it was impossible to pass a stilet between them.

A great deal of inflammation followed the operation, with profuse suppuration, and hectic fever,—which continued for several weeks and exhausted the patient very much—so much indeed that I thought at times that I should be obliged to remove the seton altogether. With tonics, however, and an invigorating treatment, after a time the hectic symptoms declined, and the suppuration abated.

In about seven weeks from the introduction of the seton, I found, on careful examination, that some firmness was perceptible at the seat of fracture. This gradually increased, and when it had sufficiently advanced, I began to remove the seton, thread by thread, until it was all taken away.

From the first introduction of the seton, until the fracture was perfectly consolidated, about three months elapsed. He was now permitted to get about gradually, upon crutches, wearing as a protection four short splints about the thigh well secured with tapes, and a roller bandage; the splints were continued for some time after he commenced walking about, and were then laid aside.

After nearly a year of great suffering, when full of cheerfulness and hope of recovery, this unfortunate gentleman again

had a fall, and re-fractured the femur at the newly united parts.

He was placed again in bed, and the limb put in the double inclined plane, and treated upon ordinary principles. At the end of six weeks after this accident, the *bone was found firmly united*, and he passed from the use of crutches to a cane,—as in every day cases—and, after all the difficulty and delay which it was his misfortune to experience, he has no more shortening of the limb than occurs in the most ordinary cases.

Case VIII. G. T., a ship carpenter, aged 44, of a vigorous constitution and athletic frame, in the year 1824 received an oblique simple fracture of the right femur—a little above its middle. After the usual antiphlogistic treatment for ten days, the limb was treated in the extended position with Dessault's apparatus. A fair amount of extension had been applied to the limb for the usual period, and upon examination at the expiration of six weeks, no union having taken place, my advice was requested.

I found the patient in good health, lying in the apparatus, which appeared to have been fairly used; the limb was, however, shorter than I have been in the habit of making, with Boyer's apparatus with the screw attached, and I found that the fractured ends had slipped by each other considerably. Free, and indeed rough motion, was given to the fractured ends of the bone daily, for several days, and the limb kept steadied in the same splint, without, however, using the least extension. Decided pain and tenderness having been induced by this treatment, I endeavored, by extension, to bring the limb nearer its proper length, but without success. It was then left in the same apparatus, four short splints having been placed in the thigh over the many-tailed bandage very tightly applied. He was also directed to use a nutritious and stimulating diet. Six weeks having elapsed without the least appearance of bony union, it was determined forthwith to introduce a seton between the fragments.

Upon making an incision on the anterior part of the thigh, down to the seat of the fracture, it was found impossible to pass a stilet or any pointed instrument between the ends of the bones, so close was their connexion. I therefore, as in

other cases, used a small gimlet, and followed it with a *bit* containing an eye, into which the seton was passed, and drawn through.

Much inflammation followed the operation, and a copious suppuration, for three weeks. When the discharge was reduced to the ordinary amount from a seton, and the wound had healed to a mere fistulous opening, he began to think that his sensations warranted him in believing that the bones were growing together. In this he was correct, for after about seven weeks the firmness of the limb had so far advanced, that it was thought best gradually to remove the seton. In eight weeks from the time of its introduction the bone was firmly consolidated.

The limb was shortened about an inch, and the foot considerably everted.

Case IX. The following interesting and unique case which recently came under my notice (1844), leaves no doubt in my mind of the existence of the non-union of an *intra*-uterine fracture.

F. G., a female infant, four months old, in excellent health, and in every other respect perfect and well formed, and nursing a very healthy mother, was brought to my house in consequence of a distortion of one of its feet. Upon examination I saw, at once, that there was a considerable mobility of the leg about two inches above the ankle-joint, and that this joint, as well as the foot, was perfectly normal. On a more careful inspection I found the tibia and fibula ununited at this point,—that the end of each bone could be distinctly felt, and that this unnatural joint was the cause of all the distortion of the foot and leg. No pain whatever seemed to attend the free movement of the unnatural joint, and the mother assured me that it had been so from its birth.

My first attempt was to render the false joint as immovable as possible; for this purpose I applied a roller around the limb, and then encased it in pasteboard, retaining this by a second roller. This apparatus was kept accurately applied during several months, and at one time I had great hope that the bones were being united, as there was a decided degree of firmness about the false joints.

As this plan failed, I tried rubbing the ends of the bones together for a few times, and then with a splint and bandage kept the limb quiet for several weeks.

No benefit following this, I applied a narrow strip of blister plaster around the leg, at two or three different times, keeping the apparatus constantly applied. Acupuncture about the ends of the bones was used, with galvanic electricity, but all to no good purpose. I regret now that I did not try scarifying the ends of the bones with a tenotomy knife, as since suggested by Prof. Miller, of Edinburgh, but it did not occur to me.

I now urged the seton, but it was some months before the mother would consent to its use; when she did consent, I felt less willingness to apply it, than in any case I ever met with. This arose from the difficulty of satisfying myself of the exact course of the arterial trunks, as well as of the position of the ends of the bones, on account of their abnormal condition, and the consequent derangement of the soft parts. It was my intention to use but one seton for the two bones, which I accomplished after much difficulty, and great care, by means of a long needle in a stiff handle with an eye near its point. After deciding upon the spot for passing the seton, it was very speedily done, and to my entire satisfaction. The infant seemed to suffer very little pain from the operation, and the inflammation and suppuration which followed it were slight. After the seton was passed, the foot and leg were secured as before.

In about four weeks the bones were quite firm, and from the general practice which I have adopted, in other cases of ununited fracture, I began to remove some threads of the seton. At this time, from what cause I knew not, violent inflammation suddenly came on in the vicinity of the seton, and involved the whole leg. This shower of trouble in a few days swept away all the uniting material, and left the ends of the bones in their original state of mobility.

The seton of course was removed at once. If it had happened that the seton had been removed before this "tornado" of inflammation supervened, it is my firm belief that this curious case would have terminated favorably.

No argument or persuasion would induce the mother to allow the seton to be re-introduced, or to try any other remedy.*

CONCLUSION.

I fear that I have already presumed upon the patience of the Academy, and shall therefore pursue this subject no further at present. It was my design to present, by way of actual demonstration of the relative utility of the seton,—a critical analysis, in a tabular form, of all the cases on record in which it has been employed; setting forth, in the unsuccessful cases, the apparent causes of the failure, and the circumstances under which it has most frequently succeeded, with the view of determining, as far as the facts on record will permit, the nature of those cases in which its use is indicated, or otherwise. This table is in course of preparation, and I shall take the opportunity of laying it before the Academy in a short time, with the practical inferences which our further investigation of the subject may justify. My present impression is this, that the seton will only succeed in those cases of ununited fracture in which the ends of the bones are in actual contact, or very nearly so; and the facts would seem also to justify the opinion that, in those cases in which the ends of the bones are not in contact, there is no operation which promises better than the resection of the ends of the fragments, and their union by means of wire.

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* P. S. This child is now about eleven years old, a fine healthy girl, with the tibia and fibula still ununited. At the request of her parents, I sawed off the ends of the tibia about three weeks since, and connected the ends of the bones together by a silver wire and canula. The wound has entirely healed by the first intention, except where the canula passes. The result will be the subject of a future communication to the Academy.

May 9th, 1851.

REMARKS ON THE IMPORTANCE OF ANÆSTHESIA FROM CHLOROFORM. IN SURGICAL OPERATIONS.

ILLUSTRATED BY TWO CASES.

By Valentine Mott.

READ OCTOBER 4, 1848.

No event has occurred in the whole range of the History of Surgery, since the discovery of the ligature by Ambrose Paré, at all comparable to the anæsthetic influence of the æthers in surgical operations. It not only robs surgery of its greatest terrors, by suspending sensibility and all consciousness of pain during the performance of operations; but it enables us to operate without the dread of any shock to the nervous system.

We are abundantly satisfied that now severe and formidable operations may be performed under the influence of those agents, which the most bold and adventurous surgeon would not have had the temerity to touch.

It is not my intention to examine into the history of the discovery of these remarkable agents, or the question of their manner of acting on the human body. For the present, we have had, perhaps, enough on these points, at least until a more extended series of results shall be recorded, to enable a purely legitimate deduction to be made.

We view the discovery of the peculiar influence of these substances upon the brain and nervous systems of human beings, and the application of them to operative surgery, as the greatest present from chemistry to surgery that ever was made, and a boon to suffering humanity of the most inappre-

ciable value. No imagination can conceive, and no tongue can tell, the accumulated woes it is destined to soothe and assuage. It far outstrips in its surgical value to the human race, any discovery of which the present century can boast.

Pain reduces all ranks to a level—it makes all men cowards. Some constitutionally and physically suffer very little from surgical operations; while others, from moral courage or religious culture, are enabled to endure great bodily torture. These, however, form but a very small item in the great mass of the family of mankind, and therefore deserve to be named only as exceptions to the great and general rule.

The dread of suffering has prevented thousands of human beings from submitting to necessary operations, and their lives have been the forfeit. Away with the stupid fanaticism that would inculcate the patient endurance of suffering, when it can be relieved. It is a Divine admonition to soothe the sorrows, and to mitigate the pains of poor human nature. And can any rational and intelligent being for a moment entertain a doubt as to the propriety of using a safe and certain agent, to abolish for a time all consciousness of pain under surgical operations?

The object of the writer is only to extol the benefit of chloroform, in cases in which the shock of the operation would in all probability be fatal without its influence. In the cases which follow, I am free to say, that I would not have ventured to perform either of them, but for the influence of this delightful agent.

From some experience in operations, I am very certain that they would both have perished long before the completion of the tedious and difficult dissection which was involved in each of them. Greater care is necessary to guard against hæmorrhage after operations performed under anæsthetic influence, from the collapse that attends the full effect of it. More time therefore has to be given, before the wound is dressed, in order that the action of the heart and arteries may be fairly restored.

Case I. A. B., a little girl from Connecticut, aged between nine and ten years, of an excellent constitution and good

CASE IV

health, was brought to me in May, 1848, with a cluster of large tumors in the neck, of different sizes, from a man's fist to that of a hen's egg, and even a black walnut, evidently making a most extensive mass of *lymphatic glandular tumors.* The whole was not much less in size than her head—reaching from behind her ear to below the clavicle and under it—extending up to the larynx and trachea, crowding them to the opposite side, and producing considerable difficulty of breathing. It had no feel of fluctuation at any part, but was elastic and without pain when handled. It had been growing for nearly five years, and commenced in one small tumor, below and about the posterior angle of the lower jaw. From this arose others, until the enormous enlargement took place, as may be seen in the drawing which accompanies this, and the morbid specimen in bottle No. 1, which is carefully preserved.

Various modes of treatment had been from time to time adopted, both local and general, popular and professional, without at any time producing the least benefit. The last that was tried were the different preparations of Iodine, locally and generally. For nearly two years she used the hydriodate of potassa as an internal medicine, and ointment of the same was rubbed upon the tumors, alternated with the tincture, without the least benefit to the enlargement; on the contrary, the size continued to augment gradually.

In a state of despair, the distressed parents brought her to me, to know if any other treatment could be recommended, or if it was possible to remove the whole by an operation with the least prospect of a favorable result.

I informed them that an operation would be difficult and dangerous, both from the unavoidable loss of blood, as well as the shock upon the nervous system. That I would not be willing to attempt it without she was put under the influence of chloroform. This I informed them was the only thing that would justify an attempt to remove the whole mass, as the shock upon the nervous system would thereby be completely prevented. That with this anæsthetic agent I felt myself warranted in performing the operation.

Inhaling the chloroform from a handkerchief, she was in a

few minutes put under the full influence of this valuable agent.

An incision was now made in the longest direction of the tumor, beginning from above and behind the ear, and carried obliquely downward and forward over the clavicle, near to the origin of the sterno-cleido mastoid muscle.

As the external jugular vein took its usual course over the tumor, I directed an assistant to compress it below the line of the incision to prevent the ingress of air, and another to seize with the forceps the upper extremity and place a ligature upon it. This was expeditiously done, the vein being considerably enlarged.

The first incision only went through the skin and platisma myoides, so as to divide the superficial jugular. The next cut the under layer of the superficial cervical fascia. It was now found that the sterno-mastoid was spread out very thin to some extent upon its outer margin over the tumor. This expanded state of the muscle might have led to the misapprehension that the mastoid was not over it. The integuments and muscle were now dissected off from the lower edges of the incision, with a view to get under it from below and on the inner sides. As soon as I found the mastoid was passing over the tumor, I at once anticipated a difficulty in its removal.

This was fully realized in the subsequent steps of the operation. The thin expanded muscle was now carefully detached from the tumor, after which the separation of it from all the adjacent parts in front and below took place. It was connected with the larynx and trachea in front, inferiorly to the inner surface of the clavicle—posteriorly to the whole extent of the carotid sheath, and behind it passed under the trapezius muscle.

An isolated tumor the size of a pigeon's egg was discovered below the level of the clavicle, with apparently the deep cervical fascia over it. On passing a double hook into it and raising it, such was its depth and attachment to the subclavian vein, that I did not think it prudent to remove it.

Although I was careful to cut through all the superimposed tissues, so as perfectly to denude the tumor before attempting

its separation, I do not think I ever met with such firm adhesions to the surrounding parts, except in malignant forms of disease. Here and there only could the handle of the scalpel or fingers accomplish any separation. At every step it seemed as if condensed and duplicated cellular membrane with arteries were entering into every interstice and irregularity of the tumor, to add to the difficulty of its removal, and to prolong the operation.

By patience and care, however, and the influence of the chloroform, a most invaluable auxiliary in difficult operations, particularly upon children, I finally succeeded in removing the enormous mass from all the contiguous and important parts, with perfect satisfaction to myself and safety to the patient.

Many large arteries and veins were tied during the operation, but the loss of blood was less than might have been expected, from the promptness and readiness with which we secured every bleeding vessel. To prevent secondary hæmorrhage, we allowed the anæsthetic influences to pass off before dressing the wound. As far as my observation has gone, I would suggest this as a proper point of practice.

When the free action of the heart and arteries had been restored for some ten or fifteen minutes, and no further hæmorrhage occurring, the wound was dressed with several stitches and adhesive straps, lint, compress, and a moderately tight bandage around the neck.

Most of the pain she complained of was a little smarting, which probably arose from the stitches being introduced after the insensibility of the chloroform had passed off. In the course of half an hour after the wound was dressed, four drops of Majendie's solution of morphine were administered, and in the evening, being restless, two more were given.

No untoward circumstance occurred after the operation, everything went on favorably, and the wound mostly healed by the first intention. After three weeks from the operation, she left for the country.

Case II. was a beautiful little boy, not quite five years old, who about a year since was observed by his mother to have a small tumor or enlarged gland on the side of the neck, below

the posterior angle of the lower jaw. It continued to enlarge gradually, unaccompanied with pain, until it attained the monstrous size delineated in the accompanying drawing, and contained in the jar before us.

At its commencement, means were taken by internal medicine and local applications to remove it. They were faithfully persevered in for months, without the least benefit; indeed the tumor continued gradually to increase in size.

It occupied the entire side of the neck, reaching from above and behind the ear, to below the clavicle. It went underneath, as well as lapped over the bone. In front it passed beyond the centre line of the larynx and trachea, crowding these parts to the opposite side—and behind it passed under the trapezius muscle. The head was considerably turned, from its immense size, to the opposite side. The appearance of it externally was irregular and lobulated. To the feel it was firm, and without the least fluctuation at any point.

The sterno-mastoid muscle could be more distinctly discovered traversing the tumor than in the former case. A number of veins were also taking the course of the external jugular, instead of one distinct trunk.

The operation was performed at one o'clock on the 26th of May, 1848. He was previously put under the influence of chloroform.

To command the whole tumor a crucial incision was made; the first extended from behind the ear to the clavicle—the second a little obliquely from the anterior to the posterior edges, so as to traverse the longest axis of the whole mass. The incisions were carefully carried through the integuments, platisma myoides, and under layer of the superficial fascia, so as fairly to denude the tumor. The dissection of the lower flaps was now commenced, tying veins and arteries as they were divided. At times it seemed for a short distance as if the separation would readily take place; but it was soon interrupted by firm duplications of cellular membrane, and vessels passing into the irregularities, which constantly embarrassed and prolonged the operation. In dissecting off the lower and anterior flap, the mastoid muscle was found to

be so incorporated with the tumor, as to make it necessary to divide it about two inches from the sternum and clavicle.

This step enabled me to get at the lower part just above the clavicle, but what next appeared was truly embarrassing, it was the deep jugular vein running through the tumor. On the inner part the common carotid was dissected perfectly bare for several inches from the tumor, but the vein was so imbedded in the mass that it was totally impracticable to save it.

For a time I left this part of the dissection, and turned my attention to the separation of other parts, hoping that I might be saved, perhaps, the division of it, but there was no alternative left. I therefore seized it with the dissecting forceps, divided it, and a ligature was instantly placed below the forceps. The upper end was readily compressed by the finger of an assistant while the dissection was continued.

This being done, I separated the lower portion of the tumor from the remaining part of the deep jugular, the tumor here having destroyed by pressure the sterno-hyoid and sterno-thyroid muscles. It was then detached from the upper and inner edge of the clavicle, to the anterior margin of the trapezius muscle. Getting under it in this way, it was more readily and safely detached from the parts below. On dissecting it from over the scalenus anticus muscle, I carefully protected the phrenic nerve. The posterior and upper part was now dissected from over the mastoid process, and turned down. A portion of the tumor was now seen to pass under the mastoid muscle, and to become so completely incorporated with it, as to make it proper here also to divide the muscle again, in order to proceed with the entire removal of the whole mass. This was done, leaving the middle third of the muscle connected with the tumor.

The anterior and upper part was now separated from the side of the pharynx and larynx, but it was found after dissecting it from the common carotid opposite the thyroid and cricoid cartilages, that the deep jugular could not be safely detached; we therefore applied a second ligature to it about an inch below the angle of the lower jaw, and divided below it, leaving several inches of the vein in the tumor, as may be

seen in the preparation. After this the whole mass readily came away.

In the course of this dissection, tedious and difficult, more than twenty ligatures were applied to arteries and veins. The quantity of blood lost during the whole operation did not exceed six ounces, several thought four or five.

After removing several small tumors from along under the trapezius, he was allowed to come from under the influence of the chloroform, before the wound was dressed, in order to favor the bleeding from any arteries that might remain untied. When all seemed safe, the wound was closed with several stitches and adhesive straps; lint compress and roller moderately tight completed the dressing.

As soon as the anæsthetic effect of the chloroform passed off, he complained of a little smarting, but did not make any particular moan until the stitches were introduced. In the course of the first hour after the wound was dressed, he took three drops of morphine to allay the smarting—and in the course of the night two more. No shock nor collapse attended the going off of the chloroform, either immediately or remotely. It was surprising and gratifying to witness this wonderful fact, and particularly to some of us accustomed to perform operations upon children and infants. In both these cases the influence of the chloroform was kept up for about an hour and a half.

To be able to put the brain and nervous system in such a state of insensibility and quiescence, as to be totally unconscious, and then lastly, after the loss of blood, and the completion of the operation, that there shall not be any collapse seen or felt by the patients, are among the wonderful facts of the present age.

He passed the night following the operation very comfortably, having a full amount of sleep. On the second day he took some aperients, and every care was now given to prevent an undue degree of inflammation. No fever occurred to require any other treatment. On the sixth day from the operation, the wound was dressed, and suppuration being established about the stitches, they were taken away. Most of the wound was united by the adhesive process; the ligatures

came away in due time, and the wound regularly and kindly healed. When it had sufficiently healed, care was taken by a bandage and position in bed, to prevent the head from becoming awry. Not only was the distortion produced by the pressure of the tumor before its removal remedied, but any further inconvenience from the loss of the middle third of the mastoid muscle.

Thus have terminated two important surgical cases. To me the result has been peculiarly gratifying. In some respects they presented peculiarities that must interest every surgeon. One is, that they could not have been borne without the anæsthetic influence of the chloroform. The shock to the nervous system in my opinion would have been fatal to both. The unavoidable duration of the operation, together with the loss of blood, would have deterred every prudent surgeon from attempting the removal of these enormous masses, without the influence of this invaluable agent.

New York, June 5th, 1848.

Since the preceding cases were drawn up for the Academy, it has been my melancholy misfortune to have encountered a fatal one from the unavoidable loss of blood attending the operation. The tumor was identical in character with the preceding, and similarly situated. The removal of the mass, which was considerably smaller than either of the two before described, was difficult and tedious, as the description which follows will clearly point out. Most of the blood lost was from veins, all of which were enormously enlarged. Several were tied previously to being divided, but what blood was lost, prevented any reaction following the operation. I do not believe he lost one tablespoonful of arterial blood.

I am delighted to be able to state, that he recovered completely from the effect of the chloroform—that it acted as kindly and as favorably upon him as in any case in which I have used it, and that my confidence in it is not in the least changed.

Case III. I. M., a delicate boy, aged five years and six months, had a lobulated tumor upon the right side of the

neck, which had been growing for about one year before he came under my notice. It commenced a little below the ear, and slowly and regularly increased in size. It extended from above and behind the lobe of the ear to the clavicle and under it—backward beyond the edge of the trapezius, and in front over the larynx and trachea. It had a soft elastic feel, but did not fluctuate at any part. The external jugular was enlarged and running over the surface, as also a number of other superficial veins distended far beyond their natural size. Two large veins in front, opposite each side of the trachea, which I suppose to have been the superficial inferior thyroidal, were nearly the size of my little finger.

He had a constant hacking cough, with considerable difficulty of breathing, which were steadily on the increase, so as greatly to annoy him during the day and disturb his sleep at night. He could only lie on the affected side. All his symptoms were gradually on the increase, as well as the tumor, which led the family to seek relief for the little sufferer. All internal means and external applications having been tried in vain, the attention of the parents and friends was directed to a surgical operation.

This they were informed could be done, but they were at the same time told that it would be attended with great danger to his life, indeed that he might die on the table; that my great fear was from the loss of blood, not the shock of the operation upon his nervous system.

I told them that I would not venture upon the operation without the influence of chloroform, and that by it all danger to the nervous system would be obviated, but that no skill could protect him from the loss of a certain amount of blood, and that the effect of the loss of this was my only fear. This we repeated to them over and over, and that with this fully understood, I was willing to perform the operation.

With all these things fully before them, they desired that the operation might be performed. It was accordingly done at 1 o'clock on the 22d of August.

He was readily brought under the influence of chloroform, but the anæsthesia was less profound than in the two former cases.

One incision was made behind, and a little above the lobe of the ear, and carried down to the clavicle in the direction of the outer edge of the sterno-mastoid muscle. It divided all the structures covering the tumor, including the mastoid muscle, which was expanded to more than double its width. Nearly one half the natural width of the muscle was left upon the outer side of the incision. At the instant of the first incision being made, a number of large veins were compressed and speedily tied. When the bleeding was fully commanded, another incision was made a little below the middle of the first, over the longest direction of the tumor, towards the top of the shoulder, which also freely divided all the superimposed structures.

On dissecting off the loose tissues, a large vein presented itself, not much less than the little finger, imbedded in the mass about two inches above the clavicle, and formed by an assemblage of smaller veins. A needle and double ligature was conveyed under it, before it was cut. The trunk below was carefully detached from the tumor, and turned with the flap over the clavicle. It continued distended with blood from the several branches which went into it.

The tumor being freed from its connexions at this point, my next object was to separate it from its attachments over the larynx and trachea. On raising the inner half of the mastoid muscle, about an inch from the line of the first incision through its middle, the deep jugular vein presented itself lying upon the tumor. This was cautiously separated, tying in the course of this dissection several small branches with two ligatures a short distance from the trunk. It was now drawn with the mastoid muscle towards the trachea, while the tumor was separated from the points underneath it. The deep jugular only was remaining over the tumor.

The mass being now dissected from over the larynx and trachea, a firm portion of it passed underneath the right side of the upper bone of the sternum and the sternal end of the clavicle. On raising the inner portion, the fingers could be insinuated partially beneath it, so that the innominata could plainly be felt. The other portion of it below the junction of the clavicle, and no small distance along this bone, seemed so

firmly attached to the subclavian vein, that I deemed it most prudent, after a careful inspection, not to attempt to remove it. It was therefore cut across at this point, leaving a small portion below the level of the sternum and clavicle. It was now seized with a double hook, raised up, and a strong double ligature passed around it, near the middle, and tied firmly. From rising and falling during respiration, I believe it was also attached to the pleura.

This done, the tumor was readily removed. Under the mastoid muscle, below and behind the ear, several smaller tumors were separated and taken away. Also from under the edge of the trapezius behind.

As soon as these were removed, he was permitted to be relieved from the anæsthesia of the chloroform. From the great care taken to secure every bleeding vessel during the whole course of the operation, not one continued to discharge blood. After waiting fifteen or twenty minutes the wound was closed with several stitches and adhesive straps, and a light dressing applied.

A small quantity of brandy and water was given him, but he refused to take much. He was now removed into bed, wrapped in blankets, and bottles of hot water placed near his back and feet. His skin was cool and moist—pulse very small and frequent. He did not vomit after the operation, though whilst it was being performed he had several turns of unloading his stomach. He had been permitted to eat, contrary to my wishes, a short time before the operation was performed.

In less than two hours from the operation his extremities became warm, with more action of the heart and arteries. This in a short time was followed by restlessness, for which five drops of morphine were administered. In a short time he fell into a quiet sleep, for two hours. A profuse sweat followed, and he gradually sank and expired in about seven hours from the operation. He retained the full possession of his mind until the last moment.

Long before chloroform was known, or used, we have met with cases in which death has followed operations, before

the wound was dressed, and a few minutes after, and at other times in a few hours.

As in this case there could be no shock upon the nervous system, his death, in my opinion, was purely one from the loss of blood. The quantity lost has been variously estimated, but the average would be from eight to ten ounces.

August 28th, 1848.

OF LACERATION OF THE CORPUS CAVERNOSUM PENIS, COMMONLY CALLED FRACTURE OF THE PENIS, ILLUSTRATED BY TWO CASES.

By Valentine Mott, M. D.

read december, 6, 1848.

Among the lesions of important organs, I am not aware that there is anything in the standard works of surgery similar to those I propose to relate in this paper. The *member* to which they are confined is liable to a great variety of serious and indeed formidable maladies and injuries, with which all practitioners are more or less acquainted. But few if any have met with similar cases, which induces me to lay them before the public.

My object therefore in relating them, is to have them preserved, as also to allay any great alarm that might be excited in the mind of the surgeon, and quiet the fear which is certain to be awakened in the patient.

The affection to which I refer has been humorously styled *a fracture of the penis.* This is, however, a misnomer in surgery, as there is in the human organ no bone entering into its composition in the normal state. In a strict surgical sense, no part can be said to be fractured which has not either bone or cartilage in its constitution. The term rupture or laceration is the proper epithet by which it should be designated. The cases therefore ought to be termed a laceration of the corpus cavernosum of the penis.

A. B., a young man living in Bergen, New Jersey, having been recently married, had his wife a day or two after this event leave him to visit a parent who was ill a few miles dis-

tant, where she was detained over night. The bridegroom, on rising from his bed in the morning, found the penis in a vigorous state of distension. In his haste to dress, not being patient with this natural condition of things, and without giving a reasonable time for its energy to abate, he struck it with considerable violence against the bed-post. At the instant this was done, a noise was heard of something breaking, and at the same moment a manageable condition of the member followed. On examination, as the bed-post was found to be sound, he concluded that his own organ had suffered the injury. He therefore alarmed the family, and it was soon reported that he had *fractured his penis.*

An extensive extravasation of blood immediately followed the injury, through the entire penis, distending it to twice or more times its natural size, changing it very quickly to a dark livid hue, and presenting altogether a most frightful and disgusting aspect.

The greatest imaginable alarm was now excited in the mind of the patient and his friends. Professional aid was summoned immediately from the neighborhood, and from the novelty and urgency of the case, further advice was sought from this city. Among others sent for was the newly made bride, who on being informed of the nature of the fracture, plaintively and innocently remarked, "*that she was sure it never would have happened, if she had been at home.*"

The tumefaction of the organ continued to increase for more than twenty-four hours, until the prepuce rolled over the glans penis, as in watery, urinous, and erysipelatous infiltrations of the filamentous tissue of this part.

Strict rest was enjoined in a recumbent posture. General antiphlogistic treatment was pursued. The penis was turned up over the pubes, and cold discutient lotions were directed to be constantly applied. By pursuing this plan of treatment, the extravasated blood after a few days began to be absorbed, and after a short time was wholly removed, and the member was restored to its normal condition and usefulness.

B. C., a young and healthy man, about thirty-five years of age, unmarried, fellow of the Academy of Medicine, came under my notice on the morning of the 13th of May, 1848.

In a very considerable state of alarm, he made the following statement to me. That on rising from bed he found the penis in a state of full and vigorous erection, and being somewhat in haste to dress, applied his hand on the left side of the member, and suddenly forced it into his drawers. At the instant of doing it he heard a crack, as he expressed it, and felt something tear. At the same instant the penis began to enlarge, and in a few minutes was distended to twice the size it had in the state of a natural erection, and without the hardness peculiar to this state. As he expressed it, his impression at the instant was, that he had, as he said, fractured it, as there was a stinging sensation of pain.

Very soon after I saw it, and the size was more than double that of a natural erection, soft throughout, and of a dark purple color. The extravasated blood reached completely to the extremity of the prepuce, so that the merest point of the glans penis could be seen. On turning it a little to the right side some pain was felt at a point on the left side of the corpus cavernosum, about half an inch from the going off of the scrotum, and it was there somewhat tender to the touch.

From the statement of the patient, and the unseemly and frightful appearance of the organ, there could be no doubt that the left corpus cavernosum was lacerated.

I advised the doctor to remain quiet in a recumbent position, turn the penis up over the pubes, and use a lotion of camphorated spirits. In the evening it had increased considerably in size, the regio pubis was also much tumefied, and the enlargement had extended over the left side of the scrotum. Leeches were now spoken of; but I feared they might be followed by erysipelatous inflammation and some of its fearful consequences. I therefore advised that a bladder partly filled with pounded ice should be applied constantly over all the affected parts during the night, and the penis to be turned up over the abdomen. This was cheerfully and promptly done. The object in view was to promote the absorption of the extravasated blood, and more particularly to prevent any erection from taking place during the night, by which the extravasation would be greatly increased.

During the night, however, while asleep, the bladder was displaced and a partial erection took place, which was accompanied with severe pain at the injured part, and an apparent increase of the extravasated blood. It instantly awoke the patient, when he reapplied the ice, with immediate relief.

The next day the general tumefaction of the penis had a little diminished, but the dark blue color was considerably increased. It was now also apparent over the entire swelling of the scrotum.

He was desired to persevere in the same treatment.

On the following day the enlargement of the penis had evidently lessened, and the color of all the extravasated parts was of a still darker hue. The integuments of the whole regio pubis began now to show a livid aspect. It was now that upon passing the finger along the left side about half an inch from the scrotum, the point of lesion could be discovered.

The doctor now felt well enough to visit a few patients, and rode out in his carriage. He carefully preserved the penis turned up, and from time to time wet a cloth with cam phorated spirits and applied it over all the parts.

The swelling continued to diminish daily, and the patient gradually resumed his accustomed occupation. The whole of the parts involved in the extravasation, the entire penis, nearly all the scrotum, part of the perineum, and the regio pubis, to the extent of more than three inches from the penis, making a semicircle to the groins, presented from its very dark color, a novel and unique appearance, such as I never witnessed before in any case.

On the 10th of May from the accident, the swelling of the penis, scrotum, and pubes had in a great measure subsided, but there was still some considerable discoloration, particularly of the scrotum. The patient states that the erections are yet attended with considerable pain, and that the penis is curved a little to the side opposite the injury. To the feel as well as sight, there is considerable thickening to the extent of an inch in length by measure, of the corpus cavernosum, and it extends from the centre of the dorsum to the corpus spongiosum, very similar to what a tendo achillis presents after being lacerated.

June 5—Twenty-third day from the accident. The doctor informs me that all pain has left him, and that the extravasation has entirely disappeared, that the incurvation to the opposite side has in a great measure passed away, and that the hardness is considerably lessened. In a word, that this organ has nearly returned to its normal condition.

New York, *June* 5, 1848.

July 1. All irregularity has mostly disappeared, and the organ may be said to have resumed its normal state.

October, 1850. Since the above was written, I have met with several recorded cases, in the periodical journals, of this injury, and had one case related to me by a professional friend which was occasioned by masturbation.

CASES

OF

TRACHEOTOMY,

WITH OBSERVATIONS.

By W. H. Van Buren, M. D.,

read april 3, 1850.

The following cases in which the operation of tracheotomy was required, occurred through the contingencies of practice, in rapid succession, within the space of a few weeks.

As the true value of any surgical operation can only be arrived at by comparing and digesting the result of a large number of instances in which it has been performed, it would seem to be the duty of individuals to place upon record plain and truthful accounts of such cases as fall within their observation, in order that each may contribute his mite to the mass of experience from which only correct final results can be deduced. The present cases furnish also, as far as I am able to judge, examples of the most ordinary termination of the operation under the several circumstances detailed; and for these reasons, therefore, I have ventured to bring them before the notice of the Academy.

I. *Foreign Body in the Air-passages; Tracheotomy; Successful Result.*

James Gallagher, a healthy child, three years of age, had been eating preserved plums, for which his father was about to correct him, when, as he commenced crying, he was suddenly seized with a violent strangling cough, which seemed to threaten suffocation.

The father, very much frightened, caught the child up in

his arms, and started for the office of the nearest physician. On getting into the open air, in the course of a few minutes he began to breathe a little more freely, still, however, coughing violently at very short intervals. At first he kept his hand almost constantly at his mouth, saying in an *altered voice*, "that he was choking," and endeavoring to get his fingers, from time to time, back into the fauces. Whilst he was carrying the child, his father, to use his own words, "heard something distinctly *clicking, with a sound like a valve*" in his throat, and even thought that "he felt it moving up and down in his neck, striking," as he said, "both ways." As it came up it would seem to excite the cough: this was quite shrill, and resembled in tone that of a croup, with an occasional whoop in the inspiration; it often ended by gagging.

The physician consulted discountenanced the idea of there being anything in his windpipe, and treated the child as for croup. At the end of fourteen days, however, the symptoms still continuing urgent, the father determined to bring him to the city, from the country village where he resided, and I first saw the patient on the 5th Sept. (1848.)

At this time, according to his father's account, he had been coughing at intervals, varying from half an hour to two hours, just as badly as at first, ever since the day of the accident. He had been uniformly worse at night, and always about 11 o'clock P. M., had his most severe and long continued spell of coughing, which, in fact, would continue almost uninterruptedly until morning, when from sheer exhaustion he would fall into an uneasy slumber. His voice had been unnatural from the first day of the accident, and whilst awake there had been a constant whistling in the throat in breathing, although during sleep it would sometimes be perfectly natural. He had always, since the commencement of the sickness, swallowed both solids and fluids without complaint, and apparently with perfect ease.

On examination I found the child with a pallid look and anxious expression of countenance, and exceedingly fretful. His face, especially about the eyes, was puffy and œdematous; skin, cool and moist; and pulse more rapid than natural.

On percussion, an equal degree of resonance was found on

either side of the chest, and it was everywhere normal. The respiratory murmur was natural in every part of each lung, mingled with a slight coarse mucous rattle, and the resonance of the laryngeal stridor. Over the trachea and larynx there was heard a whistling sound, equal in inspiration and expiration, such as would be produced by any cause diminishing its natural calibre. Pressure on the thyroid cartilages always gave pain, and generally excited a paroxysm of the suffocating cough. No clicking sound could be heard in the trachea. My conclusion was that the child had a foreign body, probably a plum-pit, in the air-passages, either in the trachea, or impacted in one of the ventricles of the larynx; that the bronchi were free.

This opinion was communicated to the father, and the operation of tracheotomy recommended to be performed, at as early a period as possible, as the most certain means of relief.

After consulting with his friends, the father determined to submit his child to the operation, and it was accordingly done on the next day (6th Sept.), the fifteenth from the occurrence of the accident.

Dr. Mott, who was present with Drs. Batchelder and Metcalfe, and a number of students, also examined the patient before the operation, and recognised the presence of a foreign body.

I determined to employ chloroform, which was kindly administered by Dr. Metcalfe; the child having been deprived of food for some hours before the operation, with the view of lessening the chance of vomiting under the influence of the anæsthesia.

The patient having been placed in a proper position, an incision two inches in length was made, extending from the upper edge of the cricoid cartilage to within three quarters of an inch of the sternum, involving the skin, fatty layer, and cervical fascia. Immediately beneath this latter, and involved in its meshes, were a number of large veins, which were only avoided by careful dissection, and by pushing them on either side; this exposed the isthmus of the thyroid gland, which was divided, with the deep fascial layer surrounding the

trachea, and the tube was thus laid bare. After arresting the trifling hæmorrhage, an opening was then made into the trachea about its sixth or seventh ring, which was enlarged upwards, with a probe-pointed bistoury, as far as its first ring. A slight cough now came on by which the air was expelled with considerable force, at each expiration, through the opening, when, before two minutes had elapsed, two foreign bodies were spontaneously discharged; one, the shank of a plum stem, lodged in the external wound; the other, a water-melon seed of the largest size, was projected quite forcibly to a distance nearly a yard, towards the child's feet. The opinion was expressed by Dr. Mott, that the seed evidently came from above, *i. e.* from the larynx.

From the moment of the expulsion of the seed it was noticed that the child's breathing became perfectly free and natural, the whistling and the peculiar cough recurring no more.

Within a few minutes the edges of the external wound were brought together without inconvenience, and retained by sutures and plaster. Through the opening by which the ligatures were brought out, air was still, however, expelled on coughing, though slightly and in small quantity.

The child was kept insensible until the dressings were completed, a period in all of fifty-five minutes, and he was then carefully covered, and allowed to sleep without the slightest disturbance. No vomiting occurred.

At the expiration of an hour his manner was perfectly natural and quiet, with a strong disposition to sleep, occasionally coughing and fretting slightly, but easily quieted. Pulse 132, natural; breathing easy and natural; skin pleasantly cool and moist.

From this time the case progressed favorably in every respect. The wound healed mainly by the first intention, no air escaping from it after the seventh day, and on the thirteenth day after the operation the child was taken home to the country perfectly well.

The case related above was especially interesting to me at the time, on account of the admirable manner in which the chloroform facilitated the performance of an operation which,

upon a young child, is always more or less vexatious and troublesome.

When the question arose as to the propriety of its administration in an operation upon the air passages, I was not aware of any precedent in its use under such circumstances, but foreseeing no objection worthy of consideration, I did not hesitate to employ it; and the result was in every respect gratifying. A few days afterwards, however, I encountered the report of an exceedingly interesting case in the Western Lancet for May, 1848, in which tracheotomy had been successfully performed upon a child four years of age by Dr. W. Davidson, of Madison Co., Indiana, for the removal of a grain of Indian corn; chloroform was employed by Dr. D., with the happiest effects. "No one," says he, "but he who has operated upon the trachea, with and without chloroform, can sufficiently appreciate the value of this agent in tracheotomy." I take pleasure in stating, also, that a case has occurred recently in the practice of Dr. J. Kearny Rodgers, of this city, in which this eminent surgeon removed a cherry pit from the trachea of a child seven years of age, under the influence of chloroform, in which the result was equally gratifying.

These cases, which prove that anæsthetic agents may be employed with safety in operations upon the air passages, are the more satisfactory as they serve to dispel the objections urged against their use under these circumstances by a high European authority. Professor Miller, of Edinburgh, in the last edition of his "Principles of Surgery," under the head of "Surgical Experience of Chloroform," expresses the opinion that "in the case of tracheotomy on account of a foreign body in the windpipe, anæsthesia must plainly be abstained from," on account of "the transcendent risk of suffocation by unejected and accumulated blood."

Experience has thus far shown, in America, that the risk of suffocation in tracheotomy is not so great as was anticipated, and it furnishes us with good reason to hope that a more extensive observation will demonstrate the safety of the employment of chloroform in this as in other operations.

II. *Syphilitic Ulceration of Larynx; Threatened Death from Suffocation; Tracheotomy; Relief.*

Andrew Wheeler, a moderately robust and healthy looking man, thirty-three years of age, with depression of the bones of the nose, apparently from syphilitic disease, was admitted into Bellevue Hospital, in September, 1848, complaining of sore throat, with great difficulty of breathing, and complete aphonia.

On first entering the ward in which the patient had been placed, I was struck by the peculiarity of his breathing, and on examining into his symptoms I was soon convinced that he was in imminent danger of death from suffocation. As far as could be learned, under circumstances so unfavorable for an accurate diagnosis, his sore throat was of some months' duration, but the serious obstruction to the breathing had existed only about three weeks, and had been gradually increasing, with occasional paroxysmal exacerbations, and for the last two nights he had been able to get no sleep whatever. Expiration as well as inspiration was accompanied by a whistling sound, as of air passing through a very narrow opening, and it was effected with much exertion, as in a paroxysm of asthma. His expectoration was copious in quantity, and accompanied with very considerable choking and strangling; it consisted of tough, thick, yellowish masses, occasionally streaked with blood, and partly sinking in water. His breath exhaled a gangrenous odor, in which that of dead bone was very evident. Pressure of the thyroid cartilage caused pain. On passing the finger back into the fauces, ulceration and partial destruction of the epiglottis was readily recognised.

On auscultation, the respiratory murmur could be heard throughout the chest, but more obscurely than natural. The resonance of the chest was normal.

After a partial consultation, it was decided to try the effect of an emetic, and in case it failed to afford relief, to have immediate recourse to the operation of tracheotomy.

At the end of an hour, I was summoned in haste by the

house surgeon, and found the patient laboring under still greater difficulty of breathing than before, and presenting an aspect of the most intense anxiety; large drops of sweat stood upon his forehead, his face was pale and earthy, his lips livid, and his hands and wrists purple from stagnation of the capillary circulation and imperfect aeration of the blood. The pulse was frequent, corded, and slightly irregular; and he was constantly shifting his position, and making signs indicating his urgent desire for relief.

I sent at once for the medical gentlemen of the house, and proceeded to perform the operation on the spot, in the usual manner, the four or five upper rings of the trachea being divided.

The relief to the patient was immediate and striking; the lungs once more played freely, the aspect of the countenance and the color of the hands became natural, the intense anxiety of manner disappeared entirely, and whilst we were yet engaged in caring for the wound, the patient fell asleep. Not having a tube of proper size at hand, I kept the edges of the tracheal wound asunder by passing a ligature through each, and tying their ends over a forked stick about two inches in length. This arrangement answered its purpose well; the patient passed a comfortable night, expectorating at intervals freely through the wound. On the next day a tube was introduced about three and a half lines in diameter, which was borne without discomfort.

On the fourth day this tube was removed for the first time, and another introduced; the one removed was perfectly black, excepting the portion of it outside of the wound. The patient was doing admirably; since the operation his pulse had not exceeded ninety, the amount and character of the expectoration remaining about the same. On the twelfth day, the tube having been changed meanwhile, as occasion required, I removed it for the purpose of exploring the interior of the larynx through the wound. On the posterior surface of the windpipe, just above the wound, I discovered with the probe portion of bone, which had become almost detached from its connexions, and was on the point of falling into the trachea, when I caught and removed it with the forceps. It

was about the size and shape of one of the *ossa nasi*, and evidently a portion of the posterior part of the ossified and necrosed cricoid cartilage.

This then was evidence of the nature of the disease of the laryngeal cartilages, and accounted for the odor of diseased bone, and the discoloration of the silver tube. Other ossified points were also detected by the probe, which was passed up through the glottis into the throat. On stopping the external orifice of the tube with the finger, after it was replaced, he could, for the first time since the operation, force air through the glottis, and made an audible noise in so doing.

For some time after this date, Wheeler's condition continued to be very comfortable; constitutional treatment was employed for the syphilitic disease of the larynx, from which he seemed to derive benefit; his appetite was good, and his general health improved. His respiration remained free, but he was much annoyed by the viscid expectoration by which the tube was frequently obstructed. This was changed and cleaned daily, being immediately replaced on removal by a duplicate tube of the same size.

During the sixth week an accident occurred which was near proving serious in its results. On removing the tube one morning, as usual, the clean one happening to be mislaid, a delay occurred in its reintroduction of certainly not more than half an hour, when the house surgeon was much disconcerted to find that, from the contraction of the tracheal opening, he was unable to get it back into its place, even on making use of some force. I was summoned in haste an hour afterwards, and found the patient on the eve of suffocation; the opening had rapidly contracted to a very small size, and his symptoms were even more urgent than when the trachea was opened at first; his distress in breathing was agonizing, and the bystanders were expecting every inspiration to be his last. Without a moment's delay I introduced a curved probe-pointed bistoury, and freely enlarged the opening downwards, making sufficient room for the ready introduction of the tube, and he was again entirely relieved.

This incident in the case is instructive as showing the singularly rapid contraction liable to occur in wounds of this

character when disposed to heal, and prevented from doing so by artificial means.

Soon after this circumstance Wheeler passed from under my care, and I was unable to see him except on one or two occasions. I learned that his expectoration became more and more difficult, and that he was constantly worried by a harassing cough; hectic fever supervened, with diarrhœa, and he died, worn out and exhausted, about four months after the operation, wearing the tube and breathing with freedom to the last.

The larynx was sent to me after his death, and is now in my possession.

The points from which the ossified portions of the cricoid cartilage had exfoliated, presented deep but perfect cicatrices; the ulcerations of the epiglottis had healed entirely; and in fact, above the artificial opening into the trachea there was no evidence of disease to be found which had not been thoroughly repaired.

Below this point, however, the trachea was the seat of numerous circular ulcerations involving its whole mucous lining, and exposing its cartilaginous rings at numerous points; they were mostly about half an inch in diameter or more, some of them running into each other, and others solitary, and extending fully to the bifurcation of the trachea.

Beyond this point there was evidence of rather intense bronchitis throughout the larger bronchial ramifications, but no further.

The preparation illustrates admirably the beneficial influence of the operation of tracheotomy in disease of the larynx, by placing the organ entirely at rest, and thus allowing the reparative process to go on undisturbed.

III. *Croup; Tracheotomy; Fatal Result.*

On Tuesday, the 2d November, 1848, I was called early in the morning to see a son of Mr. C. B., 86 Madison st., in consultation with Dr. Conway.

The child, about three years of age, and moderately robust, had been attacked on the Sunday morning previous with

symptoms of croup, for which he had been appropriately treated, and it was not until Monday evening that his breathing became permanently affected, and his condition was considered alarming. He was then leeched around the throat, and took calomel and antimony, by which he was temporarily relieved, but his symptoms shortly returned with increased violence.

On Tuesday at nine o'clock P. M., I found him with a pallid aspect, a pulse of one hundred and thirty, and breathing with considerable difficulty. There was no duskiness of the complexion, or lividity of the lips.

I proposed to apply the nitrate of silver to the larynx, at twelve o'clock, and that the fumes of cinnabar should be employed in the meantime. At this hour I found the child no better; some tough viscid material had been expectorated with temporary relief, but there was now occasional duskiness of the skin and lividity of the lips.

I proceeded to apply a solution of the nitrate of silver (℈ij—℥i,) with a sponge and whalebone of appropriate size, to the interior of the larynx. This was done thoroughly four times, at intervals of from five to ten minutes. No irritation or distress of consequence was produced by the application, and but one fit of coughing, by which nothing was discharged.

I proposed to open the trachea at four o'clock P. M., if no relief should be obtained before that time. This was agreed to, and before the hour arrived I was summoned to perform the operation at once, as the child was thought to be sinking more rapidly. I found him weaker, but with no additional symptoms save an occasional wildness of manner, and the slightest possible convulsive twitching of the extremities. The dark tint of the countenance was also more marked.

With the consent of all parties, and at the urgent desire of the father, I proceeded to open the trachea, which was done in the usual manner, four of its rings being divided immediately below the isthmus of the thyroid gland.

After the momentary annoyance of the operation had passed off the breathing became much more free, and the face and lips resumed their natural complexion; the child asked, as

well as he could, for drink, which he swallowed with avidity, and also for food, which was given in small quantities.

A tube was introduced, and the patient was placed in bed with a good pulse.

He continued to breathe freely, with occasional uneasiness, and slept mostly for two hours, when, without any warning, whilst his father was listening to the sound of his breathing, and congratulating himself upon its perfect freedom, it suddenly ceased, and before he could get to the bedside, the child was dead.

No obstruction whatever had occurred in the tube; this had been carefully watched by two of my students who had remained with the patient for the purpose.

No post mortem examination could be obtained.

A

CASE OF CROUP IN WHICH

TRACHEOTOMY

WAS SUCCESSFULLY EMPLOYED.

By Gurdon Buck, Jr., M. D.

read april 3, 1850.

Samuel B———, a lad eleven years of age, residing in Brooklyn, Long Island, was attacked in the month of May, 1849, with scarlet fever, in the treatment of which calomel was freely administered. Profuse salivation succeeded, and destructive sloughing which involved the left edge of the tongue, the gums and the alveolar sockets of the lower incisor teeth of the left side, and the under lip at the left angle of the mouth. Superficial abscesses also formed beneath the scalp and upon other parts of the body. At the expiration of about five weeks from the commencement of his illness, and while gradually recovering from the debilitated condition consequent upon mercurial cachexia, he was attacked with symptoms of croup, of which he was temporarily relieved by appropriate remedies. In a few days, however, the disease re-appeared with increased violence, and notwithstanding the judicious and skilful treatment employed, it advanced steadily towards a fatal termination. Under these circumstances, I first saw the patient on the 8th July, at 1 o'clock P. M., at the request of his attending and consulting physicians, and found his condition as follows.

He lay in a horizontal position with his head thrown backwards, and breathing with great effort, each inspiration being accompanied with a loud, hoarse, metallic sound. His countenance was anxious, his pupils dilated, and his eyes had a wild expression. The voice was reduced to a whisper. The skin

was moist; the pulse, though frequent, was not irregular nor intermittent, and still retained a good degree of force. The respiratory murmur was audible and clear over the entire posterior part of the chest. The patient's situation was evidently one of imminent danger, and inasmuch as the disease had steadily advanced with only temporary abatement, during the preceding forty-eight hours, in spite of efficient treatment, the only remaining resource that afforded a reasonable hope of relief was the operation of Tracheotomy, without which it was scarcely possible for him to survive many hours. The operation was therefore decided upon without delay, and performed as follows:

A folded sheet being passed round the body confining the arms to the sides and the patient placed so as to expose the neck favorably to the light, a longitudinal incision two inches and a half in length was made over the median line by dividing perpendicularly a transverse fold of skin, pinched up between the thumb and finger of the operator and an assistant. This incision extended over the lower half of the larynx and upper part of the trachea. The subjacent layers of aponeurosis were then successively divided, and the sterno-hyoid and thyroid muscles drawn to either side. The isthmus of the thyroid body being now brought into view was partly torn across at its upper edge, and partly pushed down till the three or four superior rings of the trachea were laid bare.

After delaying till all hæmorrhage had ceased, the opening into the trachea itself was effected as follows:—A transverse slit, one fourth of an inch in length, was made between the first and second cartilaginous rings; the lower edge of the slit was then seized with a clawed forceps and a triangular piece excised with scissors curved edgeways, the incisions commencing at either extremity of the slit and meeting below at the inferior edge of the fourth tracheal ring. At the instant of perforating the trachea the air rushed in with a hissing sound, and as soon as the opening was completed, respiration was promptly established through it, and in a short time became tranquil and easy. No embarrassment occurred from hæmorrhage into the trachea at the moment of opening it, the precaution having been taken to delay the opening

until the flow of blood had ceased. The convulsive cough consequent upon establishing a new passage for the air to and from the lungs was of short duration.

The rapid transition from extreme distress and anxiety to a state of tranquil repose and comfort, was scarcely less gratifying to those who witnessed it than agreeable and welcome to the patient himself. His countenance lost its wild and anxious expression, and became calm and natural. Directions were given to wipe away promptly the viscid secretion from the wound whenever coughing occurred. At 9 o'clock P. M., we found our patient had slept quietly most of the time since the operation, and his breathing had continued perfectly easy. Fearing lest the tracheal opening should become contracted by the swelling of the edges of the wound and the accumulation of the viscid secretion around its orifice, a full sized canula of the ordinary shape was introduced and secured in place by a tape tied round the neck.

July 9th. Patient had passed a quiet night. Respiration continued easy and other symptoms were favorable. Removed the tracheal tube, and after cleansing it of the tough viscid secretion lining the inner surface, replaced it as before.

11th. Progress still favorable. The rapid accumulation of the viscid secretion upon the inner surface of the tube rendered it necessary to cleanse it twice in twenty-four hours. On exploring the top of the larynx with the forefinger passed back into the fauces, the aryteno-epiglottic folds were felt to be very much swollen, soft, and pulpy. The epiglottis itself was normal. Applied a solution of nitrate of silver (℈i to ℥i), to the larynx by means of a curved whalebone probang.

12th. Increased the strength of the solution to one drachm to the ounce, and applied it daily. The continuance of the obstruction of the larynx without perceptible abatement showed conclusively what would have been the result of the disease if the operation had not been resorted to.

14th. Some diminution of the obstruction in the larynx seemed to have taken place. In closing momentarily the tracheal opening, patient was able to breathe once or twice through the natural passage, but not without great effort.

The application of the nitrate of silver was continued till the 30th, when it was suspended for three weeks, the tube in the meantime being changed twice in twenty-four hours.

August 20th. On resuming my attendance which had been interrupted by sickness, patient was found to have improved very much in health and appearance, and to have gained flesh and strength. The condition of the larynx was as follows :—

The tube being removed and the tracheal opening closed, a few words could be uttered in an audible tone, but not without considerable effort. Respiration could be carried on through the larynx only a very short time, and also required very great effort.

With the view of exercising the obstructed parts without removing the tube, a free opening was made at the bend of the tube through its convex side which would allow the ascending column of air to pass up through the larynx in the act of expiration, and the reverse to take place in the act of inspiration, the outer orifice of the tube being closed.

With the tube thus arranged, and in situ, it was found that after inflating the lungs through the tube a very considerable effort was required to expel the air through the larynx with the external orifice of the tube closed ; thus showing the existence of obstruction to the egress as well as the ingress of air through the larynx. Patient was directed frequently to repeat this experiment himself, in the hope that the expansive pressure of the ascending column of air against the walls of the larynx might aid in overcoming the obstruction.

28th. No perceptible improvement had taken place since the preceding note. The obstruction appeared to be unchanged. Resumed the application of solution of nitrate of silver (ʒi to ℥i), and succeeded in passing the sponge into the cavity of the larynx.

30th. Some improvement was now observable. Patient could count from one to six in a clear tone of voice with the tube closed externally.

September 1st. Still further improvement has taken place. Patient could count up to thirty, and breathe a few times uninterruptedly through the larynx.

5*th*. A sudden change of weather having interrupted his improvement since the previous date, patient was now again regaining what he had lost. Ordered iodide of potassium in solution, two and a half grains, three times a day. Stopped applications to larynx.

11*th*. The improvement of the voice continued, while that of respiration did not keep pace with it; imprudent exposure to the cold wind on the roof of the house had retarded his progress. Resumed the application of solution of nitras argenti (℈iv to ʒi) to the larynx, but continued it only a few days, after which all further treatment was laid aside. Up to the present time (April 3d, 1850) the patient, who is now submitted to your examination, has continued to enjoy excellent health. He still wears the tracheal tube arranged in the way already described, and suffers much less inconvenience from it than would be supposed. He is a boy of great activity, participates ardently in all the out-door sports of boys of his age, and also attends school. With the tube closed, he can breathe eight or ten times uninterruptedly, though before completing the number considerable effort is requisite. In using his voice he closes the tube with his finger.

In the month of October following, this patient was seen, and his condition ascertained to be very much the same as when exhibited to the Academy.

AMPUTATION OF THE THIGH,

AND SUBSEQUENT

AMPUTATION AT THE HIP-JOINT,

FOLLOWED BY PERFECT RECOVERY.

By W. H. Van Buren, M. D.

READ MAY 1, 1850.

The subject of the present notice was brought before the Academy of Medicine, for the inspection of its members, at the regular meeting of May 1st, 1850, within six weeks after the second amputation, on which occasion the following history of his case was read, and the morbid specimens therein described were exhibited. At this time he was almost entirely well.

A residence for several months in the country subsequently contributed to the perfect restoration of his strength, and for the last four months he has supported a numerous family by his daily exertion, enjoying meanwhile full and perfect health.

In connexion with the cases already on record, in which amputation at the hip-joint has been successfully effected after the previous removal of a portion of the limb below, the present case will assist in demonstrating the greater safety of the operation under these circumstances.*

The unusual size and apparent tendency to reproduction in the bony growth, which rendered the operation necessary, are also features of interest in the case; whilst the method

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* There are three cases on record, besides the present, in which amputation at the hip-joint has been done after a previous amputation in the same limb above the knee, viz. that of Astley Cooper, in 1824 (*Lond. Lancet*, Vol. II., p. 96, 1824), Mayo, in 1841 (*Cyc. Prac. Surgery*, Part II., p. 182), Sands Cox, in 1844, (*Memoir, &c.*, Lond., 1845). All have been successful.

adopted in the disarticulation of the thigh, it is believed, has not been hitherto described.

121 Bleecker st., }
6th November, 1850. }

Elijah Vanderhoof, a native of New Jersey, of moderately good constitution, forty-three years of age, and the father of a family, came under my care in the month of May, 1848, with an immense tumor involving the lower half of the femur of the left side.

About twenty years before, he had wrenched his left knee in wrestling, in consequence of which he was confined to the house for a few days; and some three months after this accident he first noticed an enlargement about the knee-joint which had, since this period, continued slowly to increase in size—the tumor extending gradually up the thigh.

At the time of my first examination the tumor involved the whole circumference of the limb, extending from the knee-joint upwards as far as the middle of the femur; it was immovably connected with the bone, and measured twenty-eight inches in circumference. On its anterior aspect it was generally spherical in shape, with slight irregularities on its surface; posteriorly it was very irregular and craggy, presenting several hard, projecting, knobby eminences. It had everywhere the feel of bone covered with a thin layer of tissues. It was not tender to the touch, although there was a point where the skin had recently assumed a dusky red appearance which caused some complaint—apparently the result of simple tension of the integuments.

There were no large veins observable on its surface. The tumor was more prominent on the posterior aspect of the limb than elsewhere, and seemed to terminate abruptly about six and a half inches above the condyles of the femur; anteriorly it shelved off more gradually, and extended apparently some four inches further upwards. The knee-joint was but slightly movable, and its motions were not accompanied by pain.

During the past year he had suffered almost constantly from a dull aching pain in the tumor, which was invariably

more severe at night, and in damp weather. This pain seemed to be gradually increasing in intensity, and had of late deprived him of sleep and diminished his appetite. He was also losing flesh, and had a pulse more frequent than natural.

I arrived at the conclusion that the tumor was benign in its character, and that it was most probably an exostosis of the femur; and also that the pain was caused by the tension of the soft parts—more particularly of the nervous trunks—resulting from the growth of the bone, which seemed to have been of late more rapid than formerly.

This view of the nature of his disease I communicated to Mr. V., informing him also of the probable continuance of its injurious influence upon his general health, and eventually upon his life, and advised him accordingly, as the tumor could not be removed, to lose the limb.

He consented to this course, and on the 20th May (1848), I removed the limb by the double flap operation, using chloroform, which acted well. In view of the benign character of the disease, I was anxious to save as much of the thigh as possible by sawing through the femur close to the tumor, and to effect this it was necessary to encroach upon the soft parts covering the diseased mass, in order to obtain flaps of the necessary size. The shape of the tumor enabled me to make the lower flap of full dimensions by carrying the knife close up to the portion of the bony mass which projected posteriorly. As the upper flap, however, could not be made in the same manner by transfixion, because the tumor extended so much further upwards on the front of the thigh, I made a semicircular sweep of the knife through the tissues covering the tumor, and dissected up the flap thus circumscribed, from its surface, so that when the flaps were drawn well back by the retractor, I was able to saw through the femur just above its middle, and exactly on a line with the uppermost edge of the tumor on the front of the thigh.—(See wood-cut, fig. 2.)

The femur at the point where it was sawn through was to all appearance perfectly sound. The flaps were ample and well proportioned, and looked precisely as if they had been made from a healthy thigh.

The patient bore the operation well, under the influence of chloroform.

It was necessary to administer morphine freely during the first few days following, on account of his restlessness and irritability.

Towards the end of the first week erysipelas made its appearance in the stump, and extended thence upon the abdomen, where it ceased. Previous to the appearance of the erysipelas, which at this time was usually prevalent in the city, coincident with the epidemic of typhus fever of 1847–8, the patient's skin became intensely yellow. About the tenth day, after the erysipelas had mostly subsided, the stump suddenly swelled up and became exceedingly tense, so much so that I apprehended arterial hæmorrhage within. There was no pulsation, however, and it proved to be venous. A large coagulum formed, which was eventually dissolved and discharged with the pus from the external openings which were kept freely enlarged. The purulent discharge was consequently copious, and highly colored by the blood until the twenty-second day, when the stump had assumed a perfectly healthy appearance. The patient was early put upon generous diet and tonics, and at the end of the fifth week, the ligatures having all previously come away, the stump was entirely healed. By the end of the sixth week he was again engaged in his occupation—that of a shoemaker—and not materially altered in appearance by his sickness, which I attributed mainly to his early use of supporting treatment.

The tumor on examination presented a magnificent specimen of true osteo-cartilaginous exostosis. Its periphery was everywhere covered by a layer of fibro-cartilaginous material varying in thickness from a line to more than half an inch, filling up its anfractuosities and giving it a much more uniform appearance of surface than it has at present after maceration. It is now exceedingly irregular in outline, covered by rounded knobs and craggy stalactitiform projections (see wood-cut). The condyles, it will be seen, participate in the alteration. The weight of the tumor when recent was thirteen pounds. The soft parts covering the bony mass were to all appearance perfectly healthy, with the exception

of the alteration consequent upon the pressure of the tumor, and their change of position. The nervous trunks, particularly the popliteal and personal prolongations of the sciatic, were observed to be thicker than natural, and had evidently, by their elongation, been subjected to very considerable stretching.

From the early part of July, 1848, the period at which he resumed his ordinary occupations, until the same month in the following year, I saw my patient but two or three times. He was enjoying very good health, and complaining of nothing except a very trifling occasional uneasiness in the stump or in the toes of the amputated limb. As this was not an unusual occurrence, I attached but little importance to his complaints.

In the latter part of July, 1849, he called upon me, complaining of increased pain which was now located entirely in the stump, and on careful examination I found to my regret that there was an evident enlargement of the remaining extremity of the femur, apparently of the same nature as the original disease.

From this time I saw him more frequently, and on each occasion he complained of more pain in the stump, and I recognised an increase of size in the disease. Little was done for him besides the application of cold, and subsequently of narcotics, to the seat of the pain. Early in the winter he was compelled to resort to the habitual use of opium in order to render the pain endurable, and meanwhile I fancied that an enlargement of the bone could be detected, extending almost as far upwards as the great trochanter.

His health now began to fail a second time, under the combined influence of the constant pain, and the habitual use of opium. He grew irritable and sleepless; his digestion became impaired, and he again began to lose flesh.

In the absence of all other remedies, I hesitated about recommending a second amputation higher up in the continuity of the femur, as there was no absolute certainty that the whole of the disease could be thereby removed, and if it could be entirely removed, I could give my patient no assurance that he would not suffer from a return of the disease in

the portion remaining. Under these circumstances, as there existed an obvious necessity for interference, I felt myself justified in recommending him to submit to the disarticulation of the remaining portion of the femur.

Under the influence of his increasing sufferings my patient did not long delay his determination to undergo the proposed operation, and accordingly on the 21st of March, 1850, with the assistance of Drs. James R. Wood, Metcalfe, Isaacs, and Cunningham, and in the presence of Professors Mott and Parker, Drs. Batchelder, Markoe, and a number of my medical friends and students, I removed the disease by amputation at the hip-joint.

Of the mode of operation I will speak more fully hereafter; suffice it to say at present that it was satisfactory in all respects; the artery was admirably commanded by Dr. Wood, by compression at first against the pubes, and afterwards in the anterior flap, whilst the hæmorrhage from the posterior flap was temporarily arrested by Dr. Isaacs, who also managed the limb, until the bleeding points were secured by ligature. By careful examination it was ascertained that not more than ℥x of blood were lost. About twenty ligatures were required.

The influence of the chloroform, which was managed entirely by Dr. Metcalfe, was perfect in every respect.

The flaps, which fitted well, were brought together with but little delay, and retained in contact by six points of suture and intervening straps of adhesive plaster; light but sufficiently firm compression was uniformly effected by a single roller.

At the time the patient was placed in bed, which was about forty-five minutes after he commenced inhaling the chloroform, his pulse was sixty-eight; he was then partially conscious. Half an hour afterwards, when his consciousness had entirely returned, it was seventy-five, and perfectly natural in quality.

He exhibited no evidences whatever of shock from the operation.

Drs. Moore of Indiana, and Doneghy of Kentucky, my former pupils, remained attentively with the patient during

the ensuing twenty-four hours. By the third day his pulse had increased to one hundred and twelve, but on the following day it was again below one hundred, which it has never since exceeded. It was found necessary to continue the opium, as before the operation, but not in any large quantities. After the third day he was allowed to use soup, and gradually to resume a nutritious diet. The dressings were not removed until the seventh day, and at this time the edges of the wound were entirely united, except at the angles, which had been purposely kept open. Subsequently one or two other points discharged slightly. The oozing had been, up to this time, unusually free. Quinine was administered after the tenth day, in order to provide against the suppuration which, from so large a wound, I anticipated would be very copious; but, although about this time his pulse began to be a little more frequent, the amount of the discharge in twenty-four hours I do not think at any time reached ℥ii. So moderate was it in fact, that, taking into consideration the very slight amount of constitutional excitement after the operation, and the absence of all evidences of inflammation in the stump, I have but little doubt that at least nine tenths of the wound healed directly by the first intention.

On the twenty-first day, before which time the ligatures had been all removed, our patient sat up in a chair, with but little assistance, whilst his bed was being made.

He suffered occasionally from colicky pains in the bowels, occasioned probably by his opium, and required two or three times a moderate dose of castor oil with laudanum; otherwise he has not experienced an unfavorable symptom since the operation to the present time, a period of not quite six weeks; and I presume that those gentlemen now present, who saw him at the time of the operation, will remark but little change in his appearance since then. There is still a trifling discharge from the angles of the wound, and a slight tenderness of the stump, which latter is also mainly owing to the morbid sensibility caused by the habitual use of the opium, which he has not been able as yet entirely to relinquish.

I propose to send him into the country for a few weeks to confirm his cure.

Remarks.—With regard to the use of chloroform in this case, the first successful operation of the kind in which, I believe, it has been employed, I can say nothing that does not confirm the immense value of the new remedy. It will be noticed that no shock whatever to the system followed the operation—the most severe of its kind that can be done on the human body; and, in view of its value in this respect, its employment is fast becoming a matter of absolute duty to the operating surgeon, whilst its power of annihilating pain, and the terrible consciousness of mutilation, no one can fully appreciate who has not personally experienced its admirable influence.

In the present case some delay occurred in bringing the patient under the full influence of chloroform (which, as usual, was administered in an adjoining apartment), on account of the slowness of respiration, and irregularity in the action of the heart seemingly produced by it. These symptoms, which would have caused any one less skilled in its use than Dr. Metcalfe to hesitate, and perhaps to have deprived the patient of its aid, were in great part owing to the patient's familiarity with the influence of the opium, which, like the habitually excessive use of alcoholic stimulus, appears to interfere to some extent with its successful administration. As it was managed, however, its ultimate effects could not have been happier in any respect.

The question may be asked, as the tumor for which this second amputation was done, was a reproduction of the original disease, on what ground was the diagnosis founded that it was a simple exostosis, and not a malignant growth? I confess that the return of the disease was a suspicious circumstance, and also that the singular variety, as well as the comparative rarity of malignant tumors of bone, have united to render the question of their diagnosis not a little obscure.

From the want of more certain knowledge with regard to their mode of development and histological peculiarities, it is sometimes impossible to arrive at a diagnosis as to their true nature, even under the microscope.

In the present case, the nature of the disease was inferred from the uniform hardness of the tumor, its weight, and its

striking and fanciful irregularities of outline; and it was confirmed by its slow growth, the absence until recently of pain, and its nature, the absence of enlarged superficial veins, and also of the evidences of cachexia which generally accompanies malignant disease in its advanced stages. It will be observed that no one of the symptoms enumerated can be considered pathognomonic of benign, or of malignant disease—if we except perhaps the peculiar irregularity of outline, and to this sysmptom I would attach more importance than to any of the others as distinguishing an exostosis from a malignant tumor of bone. And yet in many cases of the osteo-cartilaginous variety of exostosis, like the present, where the irregular outline of the bone is covered with a variable layer of cartilage, this characteristic is by no means well marked.

The slowness of growth of the tumor would seem to be no evidence of absence of malignity, nor yet its origin in mechanical violence. Uniform hardness of surface is also met with in soft cancer of bone, where the disease commences in its medullary cavity, in the earlier stages of its growth; it is also met with in enchondroma under the same circumstances, and in other non-malignant growths when they take their origin within the cavities of the bones. As to the matter of pain, about which so much is said in connexion with the diagnosis of malignant disease, I do not hesitate to say that its presence or absence affords no useful indication whatever as to the nature of the disease in tumors connected with bone. I shall not trespass upon the attention of the Academy at present, by attempting to demonstrate the truth of this assertion, but will merely remark that in the careful analysis of more than one hundred and fifty cases of malignant disease with regard to this point, I was driven to the conclusion that there is no variety of pain strictly characteristic of any form of cancer, or of the disease in general. In a large proportion of the cases of soft cancer, there was total absence of the symptom; but occasionally, when a nervous trunk was subjected to tension, the pain was excessive—it was always, however, attributable to the tension or to inflammation excited thereby in the surrounding tissues, and never to the tissue of the tumor itself. This conclusion accords entirely with the

anatomical fact that nervous filaments have never yet been detected in the substance of these heterologous growths. Hence I would offer the conclusion that the pain experienced by my patient, which seemed solely to account for the deterioration of his health, in no way invalidates the diagnosis as to its nature—although the disease did return after its removal. And I presume that this conclusion will be confirmed on examination of the reproduced disease on the upper half of the femur last removed.—(See wood-cut, fig. 2.) The specimen previous to dissection was successfully injected by my friend Dr. Isaacs, to whose kindness I am indebted for its preparation. It will be perceived that the appearance of the disease corresponds with the description already given of the tumor first removed. It is of a uniform bony hardness, and very irregular outline, involving the lower end of the bone, and extending upwards towards the trochanter. One spicular prolongation projecting towards the joint, on its anterior surface, was grazed by the knife in cutting out the anterior flap; had this flap been half an inch longer, the knife would have been caught behind this bony projection, and the operation unavoidably delayed. This danger was partially recognised beforehand.

As this specimen has been preserved in the wet state, the layer of the fibro-cartilage on the surface of the bone can be recognised.

In removing the layer of muscles covering the disease, it was noticed that the *sartorius* and *rectus*, and most of the *adductor* group were closely attached by their cut extremities to the enlarged bone; the first mentioned muscle, in fact, was inserted by a well marked tendon, and was noticed before the operation to act strongly as a flexor of the stump. The muscles preserved their volume fairly, although they had evidently undergone some degree of fatty atrophy.

The femoral artery was pervious, and apparently of full size up to a point about two inches from the extremity of the bone, where it became transformed into a fibrous cord.—(*b.*) The sciatic nerve was considerably enlarged, particularly at its extremity, where it is closely adherent to the surface of the bone, occupying as it were a valley between two projecting

Fig. 1.

crags of bone, by the growth of which it was constantly subjected to increasing pressure.—(*a*.) This was the spot upon the stump to which most of the pain was attributed before its removal.

A few remarks upon the mode of operation, and I will close.

Amputation at the hip-joint is one of the few operations in surgery, in which rapidity of execution would seem to be absolutely essential to success. Its great dangers arise from

shock to the system, and loss of blood. The former, it has been seen, was to be obviated by the use of chloroform. It was therefore left for me to choose the method of operating by which the limb could be removed with the greatest rapidity, and therefore with the least loss of blood.

It is almost unnecessary to remark that the numerous modes which have been recommended for the disarticulation of the hip-joint are merely variations of one of the three principal methods of performing amputations in general: i. e. of the circular, oval, or flap operations. The former, the operation recommended by Abernethy, for obvious reasons, has fallen into disuse. The oval method, that proposed by Cornuau and Malgaigne, a safe, but necessarily a slow operation, has never to my knowledge been employed in a successful case. The flap operation has been justly preferred by the majority of operators and writers, in one of its varieties, viz. by lateral or by antero-posterior flaps, either single or double.

Twelve years ago I was taught in Paris that the operation by antero-posterior flaps was the preferable mode; at present both of our teachers of surgery in this city recommend the method by lateral flaps, and it has also been employed in the three instances in which this amputation has been performed here.* In favor of the former mode we have the names of Beclard, Baudens, Sedillot, Liston, and Fergusson; whilst Larrey, Guthrie, Lisfranc, and Mott have preferred the lateral flaps. A large proportion also of the favorable results on record have followed the use of the lateral flaps, amongst which is the last successful case of which I have any knowledge, that of Mayo, of London; this circumstance is explained, however, by the fact that this method has been the one most generally resorted to, as the same proportion holds good in its unsuccessful employment.

In this division of opinion, on a matter which I believed to be of so much importance to my patient, I betook myself to the dead body, and divesting myself as much as possible of all preconceived prejudices, determined to ascertain, by repeated trials, by which method I could most safely and

* By Drs. Mott, Hoffman, and Buck.

quickly disarticulate the thigh. I tried the circular, oval, and flap operations in all their varieties, some of them repeatedly, and finally adopted a method which I have not met with elsewhere, and which I shall therefore attempt to describe.

The patient, already under the influence of chloroform, being placed on his back upon a table, with the buttocks projecting beyond its edge, the limb to be removed is committed to an assistant previously instructed as to its management,—the other limb to a second assistant, who carries it with the scrotum and penis as far as possible to the opposite side, and who also steadies the pelvis; the external iliac artery is then, at the word, forcibly compressed against the horizontal ramus of the pubes by the principal assistant; and the surgeon, standing on the outer side of the limb, transfixes it with a straight narrow knife ten inches long, entering its point about an inch above the great trochanter, grazing the head or neck of the femur, if possible, as it passes in front of it, and pushing it through the integuments near the anus at a point diametrically opposite to its entrance, cuts out an anterior flap in the usual method, at least six inches in length. Meanwhile the principal assistant, passing one hand into the wound behind the knife, grasps the flap, and with it the artery before it has been divided, and as soon as the division is completed, with both hands carries the flap upwards as forcibly as possible. The surgeon then, slightly kneeling, carries the knife beneath the thigh to its inner side, as in a circular amputation, and placing its heel on the integuments at the internal angle of the wound, sweeps it firmly across through the tissues on the back part of the thigh, cutting with a slightly sawing motion down to the bone, and joining the two extremities of the first incision. The long knife is then immediately relinquished, and with a large straight scalpel, the femur being forcibly abducted, the capsule of the joint is laid open as near as possible to the acetabulum, the round ligament divided with the rotator muscles inserted into the trochanter, and the fossa at its base, the assistant managing the limb so as to keep these parts successively on the stretch, and the operation is completed.—(See wood-cut.)

A large compress or folded towel is then immediately

applied to the surface of the posterior flap, by the assistant who drops the amputation limb, and the arteries are secured in detail. The femoral and profunda, if well commanded, may be left until after the branches of the gluteal and ischiatic arteries have been secured in the posterior flap.

The arteries should be tied as rapidly as security will allow—the flaps brought together with as little delay as possible,* and the patient removed to his bed, where, if his condition allow, he should be left entirely undisturbed to recover from the influence of the chloroform.

Fig. 2.

* "I consider the success of the operation to depend very much upon the quickness with which it is performed, (not only on account of hæmorrhage, but) to avoid the shock the constitution receives from the continued exposure and irritation of so large a surface in the immediate vicinity of the trunk of the body."—*Guthrie.*

I employed the operation thus described in the case of my patient, and I may truly say that there was not a circumstance occurred in its progress that I could wish to have happened differently. Indeed, with such assistance as I enjoyed, it could not have been otherwise.

I will add a few words with regard to what I believe to be the advantages of this mode of operating. It is nothing more than a modification of Liston's operation with antero-posterior flaps, in which the posterior flap is made by cutting from without inwards towards the bone, instead of in the opposite direction, the disarticulation of the thigh being left to the last.

The disarticulation of the head of the bone, and the detachment of the great trochanter from its numerous connexions, is confessedly the most difficult part of the operation ordinarily, and that most liable to cause delay. In the operation by lateral flaps, cutting around the great trochanter, whether effected in making the first flap as by Lisfranc's method, or in the last, as by that of Larrey, is a clumsy process in the most skilful hands. The descriptions of these operations read smoothly enough in the books, but they are far more difficult in execution.

Dr. Mott performed a modification of Larrey's operation by lateral flaps, previously tying the artery in the groin, a measure now generally considered unnecessary. I have in years past repeatedly assisted him in performing this operation upon the subject, but with all his tact in such matters, the superiority of the method was not so striking as to induce me to follow it without further investigation, and I was exceedingly gratified to hear him express the opinion, that if he ever had occasion to repeat the operation, he should adopt the process which I have described.

One principal cause of the difficulty in disarticulation and in the detachment of the trochanter is, that it is generally attempted with the long hip-joint knife, an instrument admirably calculated for making the large flaps by transfixion, but the most awkward possible for cutting out the head of the bone, or around the trochanter. To relinquish the long knife, disjoint the bone, and detach the trochanter with a scalpel, and then resume it to complete the remaining flap, would be a

more sensible, but necessarily a tedious plan. It would certainly be better to make the flaps with the long knife, and then to complete the operation with an appropriate instrument.

Both of these advantages are secured by the method described; and moreover, in it there is no necessity, as in the ordinary operation by antero-posterior flaps, of transfixing the thigh so very near to Poupart's ligament, for as the bone can be disarticulated with an appropriate instrument, and always with certainty and celerity, it is no longer required to expose the joint so extensively in the first incision—which is the only object gained by entering the point of the knife an inch below the anterior superior spine of the ilium.

In conclusion I would state my belief, from repeated trials, that the operation described above could be invariably done in less than a minute by the most unpractised hand, at all accustomed to surgical operations, and by a little practice in less than half the time mentioned, as was the case in my patient.

The stump, you will perceive, is well formed, sufficiently full, and in every respect answers its purpose well. (See woodcut.)

Fig. 3.

ON THE

PURITY AND USE OF CHLOROFORM.

By J. T. Metcalfe, M. D.

read october 5, 1850.

Among the many brilliant discoveries due to the research and ingenuity of modern times, there is scarcely one having so important a bearing on the physical well-being of man, as that for which we are indebted to the science of medicine; and to that science, as cultivated in our own country.

The discovery of Etherization (by which term we would designate the production of anæsthesia, by inhalation of either of the vapors hitherto used for that purpose) may truly be regarded as forming an epoch, not only in the history of the healing art, but in the history of the world.

For centuries, the great desideratum had been toiled for, without success; until we had been compelled to own, that in very many instances, physical suffering, such as we can now easily and safely alleviate, was a necessity from which there was no escape, and which was only to be met with quiet endurance and resignation.

So fixed, indeed, had become the idea that it was a necessary condition of our existence, that an almost universal expression of incredulity welcomed the announcement that, in Boston, a liquid had been discovered, by inhaling the vapor of which certain formidable operations in surgery, hitherto attended with great pain, might now be performed without the consciousness of the patient, and—still more wonderful,—without the new agent leaving any injurious effects after its use.

A few enthusiasts took this, as they would have taken any other marvellous statement, for the truth; but men of less

credulous minds nearly all agreed in considering it as one of the countless devices to which charlatanism has been wont to resort, in all ages, when seeking its own account, by extorting from human infirmity the tribute money so readily and so lavishly paid to imposture and deception, by all conditions of men. This opinion of the incredulous was somewhat justified, by the circumstances of equivocal propriety accompanying the first introduction of the Letheon.

The attempt to exclude any portion of our suffering fellow mortals from a free and full participation in the inestimable benefits of the new remedy, was met with that reprobation which truly enlightened physicians have never failed to bestow on any effort to restrict within the narrow, selfish limits of a patent, whatever may have been proved useful and valuable in the therapeutical world. It affords us pleasure to state that a prompt and willing abandonment of anything like exclusive property in the newly discovered agent, followed the immediate expressions of disapprobation which arose from the entire medical fraternity of this and of all other civilized countries.

After the announcement that Drs. J. C. Warren and Hayward, of the Massachusetts General Hospital, had performed surgical operations successfully, and without the consciousness of pain by the patients, whilst under the influence of ether, in October and November of 1846, medical men began seriously to inquire how much of truth there might be in the wonderful statements promulgated from Boston, under the sanction of such distinguished names.

In an unprecedentedly short space of time, their inquiries were answered from all parts of the earth to which civilization had penetrated, that not only had the results of experience gone to confirm the truth of the facts stated by the Bostonians to the fullest extent, but that the applications of anæsthetics were far more numerous and varied than the original experimenters had ever dreamed of.*

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* As an instance of the rapidity with which etherization spread, I may state that a correspondent of the *American Journal of Medical Sciences,* writing from Vienna on the 19th February, 1847, says:—" Of late, the whole medical world

On the tenth of November, 1847, a paper was presented to the Medico-Chirurgical Society of Edinburgh, by Professor Simpson, communicating the discovery of a new anæsthetic agent, chloroform, which he asserted to possess several advantages over sulphuric ether—hitherto the only substance at all generally used by inhalation to annul pain. These advantages were:

1*st*. The necessity for a much smaller quantity.

2*d*. A more rapid and complete action. The time of the surgeon is thus saved, and preliminary excitement avoided.

3*d*. The preference given to chloroform over ether, by those who have inhaled both.

4*th*. Its greater portability.

5*th*. No special instrument is needed for its administration.

From the high position and great scientific reputation of Professor Simpson, the new agent was rapidly introduced into practice, and has been, during the past three years, subjected to many hundred thousand trials. The result of these shall be hereafter considered.

It would far exceed the limits of such a paper as I should feel justified in presenting to the Academy, were I to trace minutely the rise and progress of etherization, up to the present day. The current medical periodicals of the four years past, have rendered us familiar with the steps by which artificial anæsthesia has reached its present condition of nearly universal use in all quarters of the earth.

We have seen with what modest propriety the first operations performed under the influence of sulphuric ether vapor, by Doctors J. C. Warren and Hayward, were published to the profession—with what hope, not unmingled with apprehension, they appealed to our fraternity to enter on the investigation of the subject; and by extensive and enlightened experience prove to the world, the just value of the great American discovery. We have witnessed the prompt response to their appeal, and to that of Professor Simpson, on making known the properties of chloroform; and we know,

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here has been occupied with the new discovery. Many trials have been made with it in the hospital, and as I learn also in private practice, all of which, as far as I can learn, have perfectly succeeded."

finally, that except with a few bigoted individuals, the two agents, to the consideration of which this paper shall be mainly devoted, are deemed inferior in importance to no articles in the materia medica.*

I propose in what I shall have to say this evening, briefly to consider the following points.

1*st.* The comparative potency of the two agents.

2*d.* Their comparative safety.

3*d.* The precautions necessary in administration.

4*th.* The means to be employed in cases of suspended animation.

With regard to the first point, it is a generally recognised fact, that chloroform is a more prompt and more powerfully narcotic agent than sulphuric ether. From the researches made by Dr. Snow of London (to whom we owe much for patient and intelligent investigation of etherization), he was led to estimate their relative potency in the proportion of 1 : 10. Any one familiar with the action of the two substances will remember cases in which, when the inhalation of ether had proved tedious and unsatisfactory, chloroform had speedily and kindly produced the desired state of anæsthesia. Its more rapid and energetic action will suggest a caution in its administration, to which we shall refer hereafter.

2*d. Their relative safety.* In considering the relative safety of the vapors, we find that the subject is one on which men's minds are not so unanimous. The advocates of ether contend that it is always to be preferred, and that, in their opinion, the use of chloroform should be abandoned by the profession.

Up to the present time, there is only one well established case of immediate death from the inhalation of the ether vapor. This happened at the Hôtel Dieu of Auxerre, in France, in July, 1847. And as the propriety of adducing this case in proof of the fatal effect of ether has been questioned, I deem it right, in this place, to state the account as much in detail as I have seen it published.

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* I have here referred only to two anæsthetics, sulphuric ether and chloroform. Other agents have been used to produce insensibility, by being inhaled in a state of gas or vapor, but they have all been abandoned by the immense majority of the profession in favor of the two above mentioned.

"A man aged fifty-five, of a robust constitution, was placed under the influence of ether, for the purpose of undergoing an operation for the removal of a cancerous tumor. After an inhalation of two or three minutes, considerable agitation was observed in the face and limbs. During five minutes more the inhalation was continued, and complete insensibility was established. The first incision was performed; but the dark color of the countenance having attracted the operator's attention, the pulse was felt, and the patient almost immediately expired. On dissection, the brain, lungs and heart, the liver, kidneys, and spleen, exhaled a strong smell of ether; the blood was dark and viscid; and the lungs were, in their posterior parts, the seat of hypostatic congestion."

On referring to the anæsthetics employed, we find that the vast majority of the profession in England, France, Germany, and I may add in the United States, prefer the use of chloroform to that of ether. They believe it to possess the advantages claimed for it by Prof. Simpson, and also state their conviction that when carefully administered it is as safe as the ether. By the recorded fatal cases, we see that eighteen times as many deaths have been caused by chloroform as by ether. It would scarcely be an exaggeration to say that the former agent has been employed several hundred times as often as the latter. Hence, by the rule of simple proportion, we are not able to consider it as less dangerous than the chloroform.

In the last number of the *Medical Times* is a communication from Mr. Tracy, dentist to Bartholomew's Hospital, stating that, in that institution, chloroform had been administered seven thousand times without fatal consequences, and that it had superseded the use of sulphuric and chloric ether. The latter anæsthetic was first used by Mr. Lawrence, of St. Bartholomew's. Its abandonment by Mr. L. for chloroform, shows his opinion of the relative merit of the two agents. Chloric ether, so far as I can learn, is now only used by a few surgeons in Boston.

There is a very common impression among medical men, that no exception exists to the statement so often made, that ether has never done harm, or at most has never been fatal

since its discovery as an anæsthetic. A fair consideration of the facts above stated must show that this impression is without truth. The manner of death is precisely similar to that which has occurred in several cases of poisoning by chloroform vapor, where the warmest advocates of the latter grant that the fatal issue was the effect of the inhalation.

In contrast with the solitary case above mentioned, we have no less than eighteen well authenticated instances, in which death was fairly attributable to the administration of chloroform vapor; of these, six have occurred in England, one in the Mauritius, four in France, one in Ireland, three in the United States, one in the East Indies, one in the West Indies, one in Scotland.

I do not propose, in *this* place, to consider such cases as that of Arthur Walker, the apothecary's boy, in England, who was killed by allowing his face to fall on a towel spread on the counter, whilst in a narcotized condition from chloroform. This was a simple case of asphyxia. No one at present doubts the *power* of this agent to kill in any case, a sufficient quantity being given. It is my intention only to refer to those cases which have proved fatal when the administration has been effected by medical men. Such, evidently, are the only ones which properly come within the range of our inquiry.

The advocates of chloroform, recognising the possible dangers from its use, state, in general terms, their belief that the amount of pain spared, and the number of lives saved in the aggregate, by resorting to this anæsthetic, is a full justification for the risk we run in any given particular case. It is affirmed by some, that *all* the fatal cases have been due either to improper administration of the vapor, or to the fact, that the purity of the drug employed was not sufficiently attended to. In Scotland, where chloroform is almost universally employed in preference to ether, the article has been generally made of very great purity. One death, out of the multitude of instances in which obstetrics, medicine, and surgery have claimed its aid, certainly very many thousands—would justly, not seem to contra-indicate its employment with our Scottish brethren.

In the last number of the "*Annuaire de Thérapeutique*" of

M. Bouchardat, we find a similar expression of opinion from Soubeiran, the original discoverer of chloroform, who has naturally taken great interest in everything connected with its history, since its introduction into medicine. His investigations have induced him to conclude that unless great precautions be taken in selecting the materials for distillation, and in conducting the process, certain compound bodies, the general composition, only, of which is at present known, are apt to come over, with the chloroform. These bodies he believes to be intermediate between chloroform itself and one of the known chlorides of carbon. He says, "La présence de l'huile chlorurée, en si petite quantité qu'elle soit, même dans le chloroforme obtenu avec l'alcool, a une influence des plus marquées dans l'emploi de ce corps; c'est à elle qu'il faut attribuer le plus souvent le malaise, les nausées et les vomissements causés par l'inhalation chloroformique."

This opinion of Soubeiran is confirmed by Professor Gregory, of Edinburgh, in a very valuable paper published in the May number of the *Monthly Journal of Medical Science* for 1850. In the article referred to, the fact is stated, that for a week, Dr. Simpson and his assistant Mr. Carmichael were kept in a state of continual anxiety, by the occurrence in all their puerperal patients for whom chloroform had been used, of unpleasant symptoms, particularly of frequent pulse and other febrile phenomena. The chloroform which he had used during this time was abandoned, and on returning to that employed some time before, all unpleasant symptoms ceased.

My own experience, extending to nearly eight hundred administrations, goes to substantiate the fact stated by Soubeiran and Gregory, as to the production of headache, nausea, and bronchial irritation, by the use of the impure liquid. With our present limited experience, it would scarcely be a matter of probable inference to attribute the fatal cases, wholly, to this cause. That some deaths may have been thus brought about would seem by no means unlikely; but in several of the eighteen already mentioned, I think there can be no doubt that there was neglect of some of the precautions which subsequent experience has taught us strictly to observe. In some cases which I believe to be of exceeding rarity, I

have no doubt that chloroform may be found to act as a poison on the system. The duty of our profession should evidently not be to reject its employment on this account; but to use every means to ascertain the conditions under which it is capable of producing unfavorable results, and the way to remedy the evil on such occasions. When considering the manner in which chloroform inhalation should be practised, I shall recur to this point more particularly.

With a view to ascertain with how much safety we may venture to administer chloroform, such as we find it in the shops, I have obtained about twenty specimens from different apothecaries, and have tested them; first, by the color, secondly, by the odor, thirdly, by the specific gravity, fourthly, by reaction on litmus paper, fifthly, by agitation with chemically pure sulphuric acid. As a general rule, the specimens examined have been colorless. In one or two, however, a slight tinge of yellow has been noticed. This chloroform has possessed a strong, unpleasant odor, and has excited irritation of the air passages, headache, and nausea, when inhaled. I should consider its use as highly dangerous. When kept in glass stoppered bottles, this liquid deposits around the stopper a yellowish, disagreeable, pungent substance, of the consistence of oil—perhaps some of the chlorinated oils of Soubeiran and Mialhe. In the sample presented, the specific gravity is high—reaching 1492. On allowing it to drop on a sheet of litmus paper, a deep red mark is left after evaporation, giving evidence of a considerable quantity of free acid. On agitation with one third its volume of pure sulphuric acid, a faint discoloration of the latter is observable, due to the carbonization of some of the foreign bodies in the chloroform.

In another specimen, possessing the disagreeable odor mentioned, in which the specific gravity was also above 1490, prolonged contact with chemically pure oil of vitriol produced only the faintest shade of darkening. This chloroform, however, gave an acid reaction with litmus paper; and after standing for some time, stained the cork of the phial a brownish yellow color.

At the suggestion of Professor Wolcott Gibbs, of the Free Academy, a specimen offering these characters was treated

with the peroxide of lead. By this, the disagreeable smell, previously existing, was completely removed. It was attributed by Professor Gibbs to the presence of sulphurous acid, probably mixed with some of the chlorinated oils of Soubeiran, or with the vapor of chloroform itself.*

It has been stated, as a general rule, that the specific gravity of chloroform may be taken as a test of its purity. This was lately mentioned to me by the clerk of one of our best apothecaries, as a proof of the excellent quality of the article he had sold me. The facts above stated, as well as Professor Gregory's tables, go to prove the error of this belief, and should admonish us to be careful in examining all specimens of the drug before giving it by inhalation.

The great majority of the samples examined were from the manufactory of Farr, Power, & Weightman, of Philadelphia, and, in almost every instance, were found to be very nearly pure. The specific gravity of no specimen examined was so high as 1500, nor was there any below 1484. This, in the early history of chloroform, was considered to indicate a very fair quality of the article.

With reference to the odor of such chloroform as came under my observation, it was rare to meet with any in which the fragrant, peculiarly pleasant smell of the pure liquid existed. It would be impossible in words to explain in what the difference consisted, but I have practically found it an easy matter to tell by the "bouquet," whether any of those substances were present which have appeared to cause headache or bronchial irritation. By comparing the two specimens marked I and G the difference will be readily appreciated, the latter being perfectly pure chloroform, rendered such by the process of Gregory. The former was presented to me by a gentleman who had attempted its use in a surgical operation. It caused such violent laryngeal irritation, however, as to oblige him immediately to desist from its use.

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* For the benefit of those who may wish to test the purity of chloroform, after Gregory's process, I would state, that in order to do so satisfactorily, it is necessary to have *chemically pure* sulphuric acid. This is perfectly colorless, and enables us by a faint brown tinge, to detect a very small amount of foreign bodies.

This sample is similar in odor and other respects to another specimen obtained from a dentist who seemed to consider it of good quality. His assistant has since informed me that the whole bottle had been returned to the apothecary from whom it had been purchased, on account of its having nearly strangled a lady, to whom it had been administered for the extraction of a tooth.

Another method of testing the purity of chloroform, by the sense of smell, consists in allowing a little to evaporate from a handkerchief. When the impurities alluded to exist, they are rendered more strikingly manifest by the odor remaining after evaporation.*

PRECAUTIONS NECESSARY IN ADMINISTRATION.

In the exhibition of anæsthetics for surgical operations, I consider it in the highest degree essential, that the operator should select some one whose duty it should be to give his *undivided attention* to the action of the vapor, and thus, by carefully observing the approach of any unpleasant symptoms, to take such measures as might be necessary to obviate them.

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* Since writing the above, I have seen a communication from Dr. Christison to the editor of the Monthly Journal of the Medical Sciences, in which he speaks of the chloroform purified by chemists, according to Prof. Gregory's process, as having undergone decomposition after having been kept for some weeks. One specimen, however, thus prepared by Prof. G. had remained unaltered for six months.

I have proved that *prolonged* contact with concentrated sulphuric acid may effect decomposition ; but if only sufficient agitation and admixture be employed to get rid of the carbonizable foreign matter, I feel confident that subsequent decomposition would not occur.

I would refer for proof of what I have stated to the specimen marked G, purified, as above described, more than five weeks ago. It is perfectly pure, and has been used several times lately by myself, always with the most satisfactory results.

We shall probably soon learn from the Edinburgh professor of chemistry, the cause of the difficulty complained of, and the manner in which it may be obviated.

It gives full employment to one person to attend to the proper amount of vapor administered, the state of the respiration, of the pulse, and the general condition of the patient. No one who may be led to watch closely the steps of the operation, is proper to manage the anæsthetic apparatus. I feel confident, that several unfortunate results have been due to a neglect of this precaution.

Having attended to the purity of the article, the first step should be to ascertain, as nearly as possible, the condition of the viscera. I should more especially direct attention to the state of the heart, as regards valvular disease, or the probable existence of fatty degeneration. In my own mind, the death of Mr. Badger, in England, was clearly attributable to the ease with which paralysis of the heart, due to this pathological condition, was effected. Fatty degeneration of the heart has also been recorded, in the post-mortem examination of several other fatal cases.

The existence of pulmonary emphysema, or of tubercles, unless in a very advanced stage, I should not consider to contraindicate the use of ether or chloroform. I have administered the latter with the happiest effects to patients laboring under these diseases; and in the case of emphysema, it has rather seemed to increase than diminish the facility with which respiration was carried on. To epileptic patients requiring a surgical operation, the same remarks would apply. I have never yet exhibited it to one, preparatory to the use of the knife, but have several times administered it, to cut short or prevent a paroxysm of the disease. In fine, with regard to chloroform, I may use the words of Dr. Hayward, of Boston, in writing of sulphuric ether: "I hardly know a state of the system, in which I should be deterred from using it, if necessary for a surgical operation, or for the relief of severe pain."

I consider the *position* of the patient a matter of great importance. It should, if possible, be free from constraint, and as nearly horizontal as may be. By attention to this we obviate the tendency to syncope, and allow the respiratory movements to be performed more freely and more easily than when the patient is upright. I have always observed this

precaution when rendering persons insensible to pain for surgical or dental operations.

Thus far, in the obstetrical use of chloroform, I have not found a single recorded instance in which death has ensued from its administration. This has been attributed to the comparatively slight degree of anæsthesia produced in these cases. For myself, whilst I feel that this may perhaps have been true in a few instances, I cannot think that the *degree of narcotism* is sufficient to explain the exemption which has so remarkably characterized its use in parturition. In several of the fatal administrations, previously mentioned, the quantity used has been exceedingly small, and the time so short as to prove that complete anæsthesia could not have been produced —in the ordinary way. In my opinion, the horizontal position has been the great cause of immunity from fatal results in obstetrics.

In cases where death has been caused by chloroform, it has been impossible, in every instance, to ascertain the position of the patient. It is extremely probable, however, that in twelve, out of the eighteen mentioned, the persons were *sitting* at the time of inhalation.

Occasionally, patients are met with who have to be instructed in the manner of inhaling. If left to themselves, they fail to take the vapor into the lungs in sufficient quantity to produce the desired effect, and we have those unpleasant feelings which almost always accompany an inefficient administration of the anæsthetic. In this way I have once or twice known a person resist the narcotic influence for half an hour before becoming insensible.

In surgery, it is frequently advisable, in order to avoid a shock in the way of mental impression, that the anæsthetic state should not be produced in the apartment where the operation is to be performed. It is also desirable that the patient, when practicable, should after the operation be removed to an adjoining room, in order that, on recovering from his senses, no shock may be effected by the necessity of seeing the bloody hands or instruments of the operator, or the portion that may have been removed by the knife. I am cognisant

of one instance in which alarming syncope was induced by the patient suddenly beholding his amputated limb on recovering consciousness.

It is prudent to observe the precaution already pointed out by writers, with reference to the condition of the stomach. This should be empty at the time of the operation, as nausea is thus much less apt to be induced, and the surgeon is not annoyed by the vomiting of the patient, and consequent interruption of his work.

I have never known any serious symptoms as a consequence of vomiting from chloroform, and must refer to the account of *possibility*, the dangers which have been insisted on as liable to ensue from closure of the glottis by the matters ejected from the stomach.

MODE OF ADMINISTRATION.

With regard to the manner in which chloroform should be administered, my experience has led me to adopt the plan of giving the vapor slowly from a handkerchief, the latter never being allowed to come in contact with the face, in order to insure a free admixture of atmospheric air at each inspiration. In some of Professor Simpson's papers, the contrary course was advised, viz. to give at once an overwhelming dose of the vapor, so as to avoid the stage of muscular excitement.

There can be no doubt that this plan is more dangerous than the one above mentioned. In many cases, without doubt, etherization may be safely effected by this method, but in constitutions peculiarly susceptible to the influence of the vapor (as in the case of Mr. Badger), it should not be left for the practitioner to confess that he has neglected any one of the precautions which should be observed in the exhibition of so potent an agent. My own experience has furnished several instances in which the truth of these remarks has been made apparent. Bronchial irritation, cough, and congestion of the face, which have followed the too rapid filling of the lungs with the unmixed vapor, have all disappeared, and the

anæsthetic state been kindly produced when the precaution mentioned has been attended to.

Dr. Snow, of London, still continues to advocate the use of the inhaler, and attributes many of the fatal cases to using the handkerchief. In Scotland, however, the latter article is, I think, exclusively employed, and the great immunity from fatal consequences which has hitherto attended its use, would seem to corroborate the opinion already expressed as to its superiority, or at least to its possessing equal merit with the inhaler.

I have spoken of the necessity of paying strict attention to the state of the respiratory function during the inhalation. In proportion as the system comes more profoundly under the narcotic influence, are the voluntary muscles of respiration paralysed, until when deep anæsthesia has been effected, we find that the abdominal muscles, diaphragm, and the natural elasticity of the lungs, are the exclusive agents by which breathing is carried on. When this kind of respiration is observed, I have always made it a rule (if there were any considerable degree of dyspnœa) to allow the inspiration of air alone, until the difficult breathing had been removed.

As has been generally recommended by those who have practised the administration of chloroform or ether for surgical purposes, it may be ascertained when a patient is in a proper condition for the operation, by watching carefully for the production of voluntary muscular relaxation. The occurrence of stertorous respirations is always a sign of deep narcotism, and should warn us to suspend the anæsthetic until the stertor ceases.

MEANS TO BE USED WHERE ANÆSTHESIA IS CARRIED TOO FAR.

What shall be done in instances where animation is suspended by an overdose of the chloroform? This is a most important question, but one which I think can be much more satisfactorily answered now than would have been the case a year ago.

I am able to speak of four cases in which after cessation of the breathing, circulating, and sensitive functions—in fact after

apparent death, life has been restored by a prompt resort to what should always be our first recourse, viz. PROPERLY PERFORMED ARTIFICIAL RESPIRATION.

Two cases in which this means was successful in restoring suspended animation from chloroform vapor, were recorded in a letter from Mr. Ricord, contained in the September number of the *Union Médicale*, for 1849.

In the May (1850) number of the London Lancet, was the report of another instance in which life was recalled, by almost immediately resorting to artificial respiration, after seeing the inefficiency of cold air and ammonia.

The fourth case occurred to myself, and as it may be useful in impressing what I conceive to be the great value of this course of treatment, I have thought it proper to give the circumstances in detail. They were as follows :—

On the 1st of March, 1850, I was requested by Dr. Delafield, of this city, to administer chloroform to a man fifty years of age, for the purpose of having extirpation of the left eye-ball performed, on account of fungus of the part. The man had previously been in general good health, and at the time of the operation was stout and robust. He was carefully examined to ascertain if there was any visceral disease, but no indication of its existence was detected. He had been accustomed to indulge freely in the use of intoxicating drinks.

As I have frequently remarked to be the case, in intemperate persons, he was long in coming under the influence of the chloroform vapor, which was administered from a handkerchief, with the precautions hitherto mentioned. His position was recumbent, on the back. In the course of inhalation, before complete anæsthesia was effected, there was a considerable amount of convulsive muscular action and incoherent talking.

Seventeen minutes after the vapor was applied, there was slight stertorous respiration, and complete muscular relaxation. He was also insensible to tolerably hard pinching.*

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* In this man's case there was the indisposition to respire, before alluded to. It was frequently necessary to speak to him sharply, and to excite inspiration by pressing the walls of the chest during the administration of the chloroform.

The operation (not requiring more than a minute of time) was then performed by Dr. Delafield, and was immediately followed by a smart gush of arterial blood from the wound. Whilst the incisions were being made, the patient groaned slightly, and made the automatic muscular motions not unfrequently seen, when anæsthesia is not carried to its most profound extent.

A few seconds after the globe of the eye had been removed, Dr. Van Buren, whose finger was on the radial pulse, observed that it suddenly ceased to beat. On examining to see if respiration were still going on, this was also found to have stopped. The face, previously somewhat livid, became rapidly blanched and cadaverous, and the hæmorrhage instantly ceased.

The true condition of matters was at once appreciated. To all appearances, life was extinct. The window, before which the patient's head lay, was thrown open, a towel wet with cold water was slapped in his face, his body was shaken roughly, with the hope that he might be aroused. These efforts were persevered in for about a minute without the slightest success. Not a single sign of animation manifested itself.

Happening, at this moment, to remember the two cases in which M. Ricord had been successful in restoring patients under similar circumstances, I at once applied my lips to those of the patient, holding his mouth open with my right hand and closing his nose with the left, and inflated the lungs, slowly and gently, so as to imitate as much as possible a natural inspiration. Dr. Van Buren, by compressing the thorax and pushing up the diaphragm, performed artificial expiration, whilst Dr. Markoe assisted by pressing the thyroid cartilage against the spinal column.

After some fifteen or twenty inspirations thus performed, occupying nearly two minutes, the patient gave a feeble gasp. This was simultaneous with a slight spirt of blood from the artery, furnishing the hæmorrhage referred to. Artificial respiration was continued as above described for a minute afterwards, the pulse regaining its strength and fulness, and the face resuming its natural color as we proceeded. The

hæmorrhage from the wound returned, the breathing became natural, and, in a few minutes, we had the satisfaction of seeing our patient restored to life, and of hearing him speak of the operation.

From this time, no unpleasant symptom showed itself. His convalescence was as rapid and favorable as the nature of the operation would allow, and he was soon enabled to return to his home, in an adjoining state.

From the results following the measures adopted in this case, in M. Ricord's two cases, and in that of M. Bleeck, there can, in my mind, be no doubt that our first duty in the event of suspended animation, from the use of chloroform, is to proceed *immediately* to the performance of artificial respiration.

In looking over the reports of fatal cases, I have been struck with the fact, that this means has seldom been resorted to, until all others had proved unavailing. The following is a fair illustration of the course usually adopted hitherto. It is taken from an account given of Mrs. Summers's case, the first fatal one in this country. "Ammonia was applied to the nostrils, cold water was dashed in the face, mustard, brandy, &c., applied, but she did not breathe nor exhibit any sign of life, after being placed in the recumbent position."

This case occurred during the early history of chloroform. In it, artificial respiration is not mentioned as having been employed.

In the last case, in which inhalation of chloroform was followed by death—that of Mr. Cock, of Guy's Hospital, the course adopted seems to me to have been very strange, considering what had already been published as to the means of resuscitation. It was as follows:—"Brandy, ether, ammonia, cold air, cold water, shaking, rubbing, electricity, and artificial respiration, for which purpose *the larynx was opened and a tube inserted.* If the artificial respiration, performed properly, had been tried at first, and the brandy used to bring up the rear of this host of remedies, I think the result would, very probably, have been different.

I have thought it proper to insist on the necessity of inflating the lungs, gradually and gently, so as to avoid the

possibility of rupturing the delicate tissues of the air cells. Is it at all improbable that some of the air mentioned as having been found in the veins, on post-mortem examinations, may have been owing to too powerful insufflation, in the attempt to resuscitate ?

Theory as well as practice would go to show the uselessness of filling the patient's mouth with brandy, cold water, or anything else, whilst the power of swallowing was abolished. Of no more utility would be the attempt to administer nitrous oxide or oxygen gas, as has been proposed. These can only be introduced into the lungs by inspiration. If the patient could perform this act, there would be no need of the means referred to, as the atmospheric air would answer the desired purpose.

I may mention in this place, that another means, said to have been successful in two cases threatening a fatal termination, consists in thrusting the fore and middle fingers as far as possible down the patient's throat, so as to produce irritation of the larynx. This was proposed and has been put in practice by M. Escalin. The details of the cases I am unable to give.

It would have afforded me pleasure to state to the Academy, the results of my experience in the use of chloroform and ether in the treatment of medical diseases and in obstetrics, but the length to which my paper has already extended, prevents me from more than alluding to this part of the subject at present.

I may mention that I have continued their use in parturition since the first employment of ether in this city, in October, 1847, and have had no cause to regret their administration. My testimony to their happy influence in obstetrical practice would differ in nothing from that of Channing, Simpson, Murphy, and others, whose experience has enabled them to judge fairly of their merits.*

* I have one patient who has been confined three times whilst in the anæsthetic state. On the first occasion ether was used ; on the second and third, chloroform was employed. The recoveries were all speedy and uninterrupted. The children all, at present, stout and healthy. The lady in this case, for violent neuralgic pains, during pregnancy, resisting all other remedies, has taken chloroform at least one hundred times. She is now in remarkably good health.

In one case, in which the mixture of ether and chloroform was inhaled, puerperal mania followed parturition. The patient, in this instance, was seen by Dr. Samuel Henry Dickson, and by the late Dr. McDonald, of Sandford Hall, who both expressed themselves of opinion that the attack was in no wise different from those usually seen of this disease. Recovery took place in the course of five or six months. In the somewhat violent paroxysms of mania, to which this woman was subject, the tranquillizing effect of chloroform inhalations was beautifully shown. It was repeatedly administered by Dr. Van Buren and myself, and never failed to produce calmness or rest, previous use having been nearly always made of the ordinary narcotic remedies without avail.

In puerperal convulsions, I have known it used six or seven times in New York. In every case except one (in which there was rupture of the uterus) the convulsions were materially shortened, mitigated, or altogether prevented. The cases, with the exception mentioned, have all recovered.

In the agonizing pains of dysmenorrhœa, chloroform has been found of great service. One of my patients, a young unmarried lady, has had it administered at every menstrual period, since its introduction into American practice. It has always afforded great relief to her sufferings, and has never in any way affected her injuriously.

I have several times administered it in violent attacks of colic, when the pain was so excessive as to render it desirable to procure relief as speedily as possible. In these cases (several of which were even attacks of cholera morbus), I have found the immediate tranquillizing effects of the chloroform to assist very much the action of the ordinary opiate.

In this country and in Europe, the sedative and narcotic properties of chloroform inhalation have been taken advantage of, in the treatment of delirium tremens. In Bellevue Hospital more than thirty-five cases have been thus treated, and in every instance in which the administration has been steadily and rightly conducted, the result has proved satisfactory. It is necessary with some patients, to remain by their side for an hour or two after sleep has first been procured. As often as a disposition to awaken is shown, the application of the

anæsthetic should be renewed. In most cases, a long, refreshing, sobering sleep is thus produced, and the patient opens his eyes in a rational condition. In a good many of the Hospital patients alluded to, the ordinary means, in general use, had been unsuccessfully tried before resorting to chloroform.

In conclusion, I would state that from the amount of evidence now before us, I do not think we can rightly infer that good chloroform, properly administered, is more dangerous than sulphuric ether. That it is more pleasant, more prompt, and more generally applicable, I have no doubt. With increased experience, I feel disposed to believe that we shall eventually come to understand the indications and contra-indications for its use, quite as well as we now understand those of any remedial agent of acknowledged power, in our pharmacopœia.

CASE OF ANEURISM,

AND LIGATURE OF THE

LEFT SUBCLAVIAN ARTERY,

ATTENDED WITH PECULIAR CIRCUMSTANCES.

By Valentine Mott, M.D.

READ MAY 7, 1851.

The course of the subclavian arteries after they leave the outer edges of the scaleni muscles, is the same on both sides. The artery can therefore be tied with equal facility, *cæteris paribus*, on both shoulders. The instances are now numerous in which ligatures have been applied to these vessels, but as there are some points about the following case which are novel and interesting, it is deemed worthy of record. In the seven* cases which have come under my notice, in which I have tied this vessel, this presents two points of great interest. It will show what may be done by patience and perseverance under distressing and almost desperate circumstances.

In the early part of November last, Dr. O'Reilly, an intelligent and well educated surgeon, sent a man to me with a tumor which he had very correctly diagnosed as an aneurism. It was situated below and behind the clavicle, occupying nearly the whole extent of the bone, and reaching into the axilla. It had the full character of an aneurism in the first stage, all the blood being fluid.

The history as given by the patient is the following:

James Smith, aged thirty-five years, a laborer, about five

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* I am happy to say that six of the cases have been successful. They have been on the right and left shoulder, without the scaleni muscles. The present case was under the anterior scalenus. The fatal one was when the ligature was placed within the thyroid axis of the right subclavian.

weeks since received a violent blow from a cask of lime, the edge of which struck him just below the left clavicle. It stunned him for a few minutes. For some days afterwards he suffered pain in using the arm, which extended in a short time to the scapula of that side. Several applications were made to the pained parts, treating it as a contusion only, for as yet there was nothing else manifested. About a month after the accident, a swelling was perceived under the collar bone, which gradually increased in size, attended with severe pain. As the tumor enlarged, the whole arm became tumefied and exceedingly painful. He passed sleepless nights, and his countenance bore marks of great distress.

About two weeks from the discovery of the tumor he was sent to me to be examined. The nature and character of the case being very evident, I requested by a note an interview with Dr. O'Reilly. As the artery could now be compressed by the fingers at the outer edge of the scalenus anticus muscle, and the pulsation of the aneurism entirely suspended, I proposed to the Dr. that a ligature should be applied at that point, as a probable means of cure. This was communicated to the patient, who immediately consented to anything that would even give him a temporary respite from the intense suffering under which he labored.

The reduced state of the patient's health from constant suffering, and the immense size of the whole arm from œdematous tumefaction, accompanied with an erythematic redness throughout its whole extent, with great tenderness to the touch, and an almost useless state of it, presented by no means a favorable prospect for the result of the operation. I, however, stated to the doctor that if he preferred it, I would perform the operation.

The tumor externally was mostly below the clavicle in its entire length, reaching also considerably into the axilla. It extended behind the clavicle, and penetrated below this bone to near the outer edge of the scaleni muscles. The clavicle and shoulder were raised by it.

The intense suffering of the patient from pain in the tumor and arm, together with the enormous œdema of it, and inflammation throughout its entire extent, seemed to admit of no

delay of the operation. From the aspect of the whole case, I confess I felt many misgivings as to the result of an operation, but it was every way justifiable and proper to give him the chance, though ever so doubtful and uncertain. This he fully understood, and was very urgent in his entreaties for its performance.

On the 15th of November, he was seated in a chair, with the left side to the light, his head reclining a little, and supported by an assistant. The external jugular being made manifest by pressure, an incision was made along its external margin, close to the outer edge of the sterno-cleido mastoid muscle, and carried down to the clavicle, about three inches in length. From the inner point of this another was carried along the upper edge of this bone, outward to the same extent. The integuments, including the platisma myoides, were dissected up, and the flap turned toward the shoulder. On cutting through the deep cervical fascia, the omo-hyoid muscle was exposed. With my fingers and the handle of the scalpel, the loose cellular and fatty tissues were pressed aside, and the subclavian artery readily felt and exposed just at the point where it passes from under the anterior scalenus muscle. With the aneurismal needle properly armed with a ligature, I now passed it from below upward, the artery being distinctly felt and even exposed to view.

Feeling for the artery a second time, I tried carefully to pass the hook under the vessel, but could not; but *why*, I could not understand, nor can I now comprehend. It appeared as if the artery was so firmly bound down, as to be apparently incorporated with the parts beneath. Perhaps the proximity of the aneurismal sac, and elevation of the clavicle may have had some agency in fixing the artery in this manner. I now turned the instrument, and with the hook from above downward, with great difficulty, patience, and perseverance, was enabled to pass the hook under the vessel. In turning the point upward, to enable me to get at the eye, an instantaneous gush of arterial blood flowed, which obscured all the parts. It was a torrent indeed, and filled with dismay several professional men present, who instantly left the room. At the moment of the gush, I passed the two forefingers of the left

hand into the wound, and applied the points of them over the wound of the aneurismal sac, which completely commanded the hæmorrhage. In the midst of the blood I now passed the needle, and brought out the hook and ligature from below upward.

After the hook was passed, all present felt the artery, and said they were perfectly satisfied that the ligature was under the vessel. I frankly own it was not entirely satisfactory to myself. I, however, with their full concurrence tied the ligature. The pulsation of the aneurism instantly ceased. I then took my seat in another part of the room, and requested my assistants to dress the wound. The parts around were cleansed, and the wound about to be dressed, when Dr. Proudfoot said, "there is some pulsation returned in the tumor." At which remark I went to the patient, and to my great surprise it again pulsated.

My first remark was, can there be two subclavian arteries in this case, or have we all been mistaken, and tied something else. The latter seemed the most probable. But what was it—not a nerve, for the tying it gave him no pain. The relative position of the parts was distinctly made out, and *seen* during the operation. The ligature was therefore as it appeared on a review of the parts, to be upon the subclavian artery. But in the normal state of things it could not be, as the aneurism had again pulsated. What was it that this ligature was around? I frankly own that I do not know.

The wound being now well sponged, I divided a portion of the clavicular part of the sterno-mastoid muscle, also laid bare the subclavian artery a little under the outer edge of the scalenus anticus muscle. When the wound was freed of blood, I made another attempt to pass the hook, but could not get it under the artery.

I now requested Dr. Proudfoot to pass the ligature under the artery. He made several attempts from below upward, then from above downward, but could not succeed. My son, Dr. V. Mott, Jun., then took the hook, and passing it from under the edge of the scalenus, more obliquely in the course of the artery from below, was fortunate enough in getting it under the vessel, after using very considerable force, and the

ligature was thereby conveyed around the artery. I then tied it.

The pulsation again ceased in the tumor, and it fell to the level of the clavicle, and was diminished to about half its original size. He remained in the sitting posture in the chair during the operation. On being asked how he now felt, he replied, "first rate"—that his sufferings were now greatly diminished in the arm. The wound was now dressed with three interrupted stitches, strips of adhesive plaster, lint, and a roller bandage moderately tight over the neck and shoulders. After getting into bed, his arm was loosely enveloped in cotton wadding, and he did not experience pain enough to require an anodyne. To procure rest at night he was, however, ordered to take fifteen drops of Majendie's solution of the sulphate of morphine.

In a few days after the operation the aneurismal tumor had almost entirely disappeared, and the clavicle had returned to its natural position. Very little constitutional disturbance or local suffering followed. Every day he was cheerful and happy, and when asked how he was, replied with a smile that he was well, and felt only the inconvenience of his restraint as to position and confinement in bed. The erythematic redness which pervaded the entire arm from the fingers to the shoulder gradually subsided, together with the enormous œdematous tumefaction of the whole extremity. The arm I should think was nearly three times larger than the second one, and so heavy that he had been obliged for some time to move it about with the other hand.

He was requested to take light and unstimulating diet, and his treatment was moderately antiphlogistic. On the fifth day the wound was dressed, a little discharge having appeared at the lower part of it. On removing the dressings, the wound had mostly united by the adhesive process. The œdematous tumefaction had considerably abated, and the erythematic inflammation had also greatly subsided. His sufferings had in a great measure passed away from the first hour of the operation, and he is cheerful and happy from the relief the tying of the artery has given him, and the hope and full confidence he indulges that he will recover.

He was directed to remain in bed in a recumbent posture, mostly on his back, which he strictly and rigidly carried out. The strict regimen which he practised, dispensed with almost all medication, except a small quantity of laxatives from time to time, and occasionally an anodyne at night. In a short time all parts of the wound were healed, except at the exit of the ligatures.

The second ligature (perhaps the veritable artery ligature) was removed with slight traction on the seventeenth day. On observing the noose, it is larger than from any other artery I have seen tied, although the knot was made very tight at the time of the operation; showing, conclusively, that the artery was unusually large, as it appeared to the feel, yet it must have been healthy to have healed so kindly. The arm was reduced to its natural size, and all the œdema and redness had disappeared. He was now permitted to be raised up in bed, and in a day or two after, to be got into a chair for a few hours a day. From the first week after the operation the suppurative process being well established, he was allowed a nourishing diet, and after the discharge of this ligature he was directed to take malt liquor, as the perspirations at night denoted considerable debility. The aneurismal tumor has now entirely disappeared.

His general health soon improved under this treatment. And now we gave attention to the *other ligature.* Slight traction of it gave him pain, which extended down the whole arm, following, however, the course of no particular nerve. No pain was felt except when the ligature was pulled upon, and it did not continue any length of time after. When the artery ligature was slightly drawn upon, he complained of the same kind of pain, but to a trifling degree.

This first ligature (or the other ligature) did not separate until the thirtieth day. To hasten its coming away almost daily traction was practised, and for ten days it was twisted firmly by a fold of linen. The noose of this almost equals in size that of the other ligature. During the time this was being removed he was walking about his rooms, in greatly improved health, and impatient of the confinement. In a few days after it came away, he went home, the wound healed

directly, and he resumed his former occupation, that of a laborer.

I am sure that the first ligature was not around a nerve, as the pain at the instant of its application would have been insupportable, and numbness of some part of the arm and forearm and fingers would have been an inseparable accompaniment. But neither of these effects was produced. While the ligature was beneath it, as is my custom, I pressed opposite it with the forefinger, and so did my assistants, and the pulsation of the aneurism ceased. And on tying it, the tumor ceased to beat, and as before stated I had taken my seat and requested the wound to be dressed.

Now what did I tie in this ligature? I frankly own I DO NOT KNOW. I cannot feel willing to admit that I was mistaken in the artery, when it was plainly felt, and even seen, and when too the relative anatomy was all exposed before me, and which I feel that I ought to be familiar with. But so it is—the facts I have stated—others may judge. If two subclavian arteries had ever been seen on one side, that would be my refuge.

To me this case is peculiarly interesting and instructive. First, on account of the possibility of two subclavian arteries on the same side. Secondly, the laceration of the aneurismal sac by the hook. Thirdly, my being able to command the frightful torrent of arterial blood from the opening in the sac by two fingers of my left hand. Then making a further dissection as before described, and tying the artery under the edge of the scalenus articus. And lastly, and best, and most certain of all, for the patient to recover.

May 7th, 1851.

HOSPITAL HYGIENE; ILLUSTRATED.

By John H. Griscom, M.D.,

read June 1, 1853.

The main object in soliciting the attention of the Academy of Medicine, at the present time, is to place on more durable record, and to make more extensively available for good, two extraordinary cases—one, of the propagation and extension; the other, of the treatment, of a now too common and virulent disease. Though each is, of its kind, but a single case, and hence, according to medical philosophy, might be considered as demanding further observation, to confirm the principles it inculcates; yet their nature is such, that, independent of the fact of each being a multiple case, and hence entitled to the force of several cases, they seem to the writer most fully to establish the important laws which they illustrate.

Probably no place on this continent has been so prolific of experience, especially during the past few years, in the habitudes of Typhus or Ship Fever, as the port of New York. The vast immigration hitherward from transatlantic ports, averaging, in the past four years, two hundred and fifty-six thousand per annum, has brought with it an amount of

suffering, disease, and death, perhaps unparalleled in modern times. Such have been the severity and devastating effects of this disorder, it has claimed and received the attention of the government, as well as of the profession.

It has been an occasional visitant at this port since the commencement of the present century, but until within a few years its visitations, as it now *frequently* appears, attacking large cargoes of immigrants, were fortunately only occasional. Badly infected ships were now and then noted, and the prevalence of the disorder in them was more reasonably attributable to protracted and stormy passages, during which the passengers were, from the state of the weather, obliged to confine themselves below, a great part of the time with the hatches closed, thus preventing the frequent exposure of persons, clothing, and bedding to the fresh air, and the execution of the proper measures for cleanliness and ventilation of the steerage, which are absolutely necessary for its prevention. Either this, or the criminal neglect of these matters by the officers of the ship, was the general cause of its endemic prevalence.

The same remarks, however, will not apply to the etiology of the infection, which pervades so greatly increased numbers of vessels at the present time. The history of the present irruption of the disease dates back to the famine which began to prevail in Ireland a few years ago, and which continued its devastations for a long period. So closely is the disorder believed to have sprung from, and to have been dependent upon, this terrible visitation of that unfortunate country, that in my estimation it might, with much propriety, be denominated the *Famine Fever.*

We need not stop here to attempt an explanation of the connexion between them, nor to show the nature of the dependence of one upon the other; these will suggest themselves to any one at all familiar with this class of patients; nor is it necessary to make any further comment upon the

havoc the disease has made among the members of our profession. The sad story is doubtless known to all. It will suffice for this simply to express the belief, that no shock of contending armies, in their efforts at mutual destruction, ever caused so many killed and wounded among their officers, in proportion to their numbers, as has resulted from the protracted struggle between humanity and disease, as displayed in the devotion of medical men in Typhus Hospitals during the past five years.

The first case I desire to present is one illustrative of the extraordinary intensity of the poisonous power of the infectious principle of this form of Typhus Fever, and of its occasional rapidity of action. It occurred in the New York Hospital, in the winter of 1851–52, in the building known as the "North House." This edifice is a very substantial stone structure, of two stories, exclusive of the basement, which is partly below ground. It is placed at a considerable distance from any other building, on very elevated ground; its halls are very spacious; the ceiling of the principal story is fifteen feet six inches high; the apartments are well lighted, and in the ordinary, but not always correct, acceptation of the phrase, would be regarded as *well ventilated*, though there are no artificial means by which proper ventilation can be effected, the windows and doors being the only reliance for this purpose. The principal floor contains four large, square wards, two on each side the hall, each pair communicating by a large door and window in the dividing partition, so that when these are open, the two form one great room. The upper story is divided into a larger number of, and therefore smaller sized rooms, with less advantages of natural ventilation. In this respect, however, the basement is the worst of all. As already stated, it is partly underground; the ceilings are only ten feet high; it is less accessible to the light, and it is more subdivided than the uppermost story.

This cursory description of the house is given chiefly to show that it possesses no extraordinary disadvantages as a hospital, but on the contrary, it would be regarded by almost any person who should visit it, as possessing, in elevation, general situation, and internal arrangements, advantages superior to a great majority of buildings devoted to hospital purposes. It was built in 1839, in the most substantial manner, solely for its present use, and accommodates one hundred and nine beds.

Prior to the month of January, 1852, a part of. the basement of this building had been allowed to be used for two or three years as a receptacle for sick immigrants, for whom a temporary night lodgment was required, but who were, generally, and except in cases of too great exhaustion, or too severe illness, removed the next day to the hospital expressly provided for them on Staten Island. A great proportion of the six hundred and fifty cases thus temporarily received in 1851, were of Ship Fever, and their number often amounted to double the capacity of the accommodations for them. Thus, though the patients remained but a few hours in the house, and were then removed, and the premises aired and cleansed as well as might be, yet the vacant places were soon re-occupied with a less or greater number of cases of the severest form of petechial typhus, whose persons were imbued with filth and vermin to the most nauseous degree, but which, under the circumstances, could not be removed or avoided.

In addition to these "night cases," the other parts of the building contained an average of about forty cases of typhus, which were then deemed legitimate subjects for admission—a proportion of numbers not at that time regarded as dangerous to the other patients, distributed, as they were, through separate wards.

This state of things had continued throughout the year 1851, and until the commencement of the writer's regular

term of attendance, in January, 1852. By this time the atmosphere of the whole house seemed to have become infected with the miasm; patients with rheumatism, or other benign complaints, began to succumb to its power in various parts, even in some of the wards into which no typhus cases had been admitted. Notwithstanding that thorough cleanliness, and as free ventilation as possible, were maintained, and the immigrant cases were rigidly confined to their particular apartments, the difficulty increased to such a degree, as at last to render necessary an entire change in the economy of the establishment with respect to this disease. At the suggestion of the Visiting Physicians, the Board of Governors first prohibited the reception of any more immigrant night cases; and secondly, a thorough purification of all the typhus wards was ordered. The number of patients was reduced, so as to allow one or two wards to be vacated, and successively the infected rooms were disinfected, and exposed to the external air night and day for several weeks, until the whole was renovated. It was during this operation that the particular case occurred which is presented as an example of the extreme virulence of the typhus poison, even under circumstances so favorable to its dilution and dispersion.

Four men, three of whom were masons, the other a laborer, or helper, were employed to cleanse the walls and ceilings, and whitewash them where necessary. They wrought only in the wards which had been vacated, and then only after each had been thoroughly aired by open doors and windows. These men did not enter any of the wards occupied by the sick; they had no occasion, in going to and fro, to see any part of the premises but the large halls, the stairways, and the apartments they were at work in.

They were engaged there one week. The helper passed a great deal of his time in the open air, preparing and carrying materials for the others, and hence visited the building

only occasionally, remaining but a short time in it, except when, being otherwise unoccupied, he aided the others a little at their work.

The effect of this exposure, which *à priori* would scarcely have been regarded as involving any danger, was as follows:—

McCoy (the boss,) was attacked with the disease a few days after finishing the work, was sick eighteen days, and died. Fleming, a workman, sickened five or six days after being there, was ill seventeen days, and died; and William (the laborer) was taken sick the last day of the week he worked there; was ill nearly four weeks, and barely escaped with his life. The other workman (Haight) was not sick at all.

Thus, out of four persons, thus lightly exposed to the miasm, three were attacked by it, of whom two died. They were treated at their own homes, which are represented as having been comfortable residences, and they were all temperate and respectable men. During my attendance in January, the House Physician was off duty, with the same disease. His place was supplied by the Senior Walker, until he too was attacked with it, and was obliged to leave.

In this extremity it became necessary to seek extraneous aid, and a former House Physician was called from his home at a distance, to resume the labors of the post. His ready willingness, alas! proved fatal, and in a manner strikingly illustrative of the occasional *rapidity,* as well as intensity, of the action of the poison on the human system. Dr. Colton had been on duty about three weeks, when the miasm seized upon him also, and in seventy-two hours from the incipient rigor, he was a corpse.

The mortality of the typhus cases in the New York Hospital in 1851, was about thirteen per cent.

From the contemplation of this picture of desolation, let us turn to a second and more pleasing one, its contrast in

almost every particular. This will be given in the graphic language in which it was first related to me, in a letter from the Hon. James Parker, a distinguished citizen of New Jersey, and President of the Board of Trustees of the State Lunatic Asylum.

LETTER FROM MR. PARKER.

PERTH AMBOY, NEW JERSEY, *March* 15, 1852.

DR. JOHN H. GRISCOM,

Dear Sir:—Having read your treatise on the "Uses and Abuses of Air," I send you an account of what occurred in this place some years since, and which proves the efficacy of fresh and pure air, not only in preventing, but curing disease.

In the month of August, 1837, a number of ships with emigrant passengers, arrived at Perth Amboy from Liverpool, and other ports, on board of some of which Ship Fever prevailed. There was no hospital, or other accommodations in the town, in which the sick could be placed, and no person could admit them into private dwellings, fearing the infection of the fever. They could not be left on board the ships. An arrangement was made to land the sick passengers, and place them in an open wood, adjacent to a large spring of water, about a mile and a half from the town. Rough shanties, floored with boards, and covered with sails, were erected, and thirty-six patients were taken from on board ship with boats, landed as near to the spring as they could get, and carried in wagons to the encampment (as it was called), under the influence of a hot sun, in the month of August. Of the thirty-six first named, twelve were insensible, in the last stage of fever, and not expected to live twenty-four hours.

The day after landing there was a heavy rain, and the shanties affording no protection with their "sail" roofs, the sick were found the next morning wet, and their bedding, such as it was, drenched with the rain. It was replaced with such articles as could be collected from the charity of the inhabitants. The number at the encampment was increased by new subjects, to the amount of eighty-two in all.

On board the ship, which was cleansed after landing the passengers, *four* of the crew were taken with Ship Fever, and two of them died. Some of the nurses at the encampment were taken sick, but recovered. Of the whole number of eighty-two passengers removed from the ship, *not one died.* Pure air, good water, and perhaps the rain (though only the first thirty-six were affected by it), seem to have effected the cure.

No report has been made of these circumstances, and I send this from my recollection, and the information derived from the physician, Dr. Charles M. Smith, who still resides here, and to whom I refer you.

Very respectfully,

JAMES PARKER.

A few further particulars of this case have since been derived from a statement of Dr. C. McKnight Smith, the gentleman referred to by Mr. Parker.

The ship was the Phœbe, with between three and four hundred passengers; a number of them had died on the passage. The shanties spoken of, were two in number, thirty feet long, twenty feet wide, boarded on three sides about four feet up, and over them old sails were stretched. Of the twelve who were removed from the ship, in a state of insensibility, such appeared the hopelessness of their condition, that the overseer (who is a carpenter), observed, "Well, Doctor, I think I shall have some boxes to make before many hours." "The night after their arrival at the encamp-

ment," says Dr. Smith, "we had a violent thunder-gust, accompanied by torrents of rain; on visiting them the following morning, the clothes of all were saturated with water; in other words, they had had a thorough ablution; this, doubtless, was a most fortunate circumstance. The medical treatment was exceedingly simple, consisting, in the main, of an occasional laxative or enema, vegetable acids, and bitters; wine was liberally administered, together with the free use of cold water, buttermilk, and animal broths." The four sailors, who sickened after the arrival of the vessel, were removed to the room of an ordinary dwelling-house; the medical treatment in their case was precisely similar, yet two of them died. Two of the number suffered from carbuncle while convalescing. The Dr. adds, "My opinion is, that had the eighty-two treated at the encampment been placed in a common hospital, many of them would also have fallen victims. I do not attribute their recovery so much to the remedies administered, as to the circumstances in which they were placed; in other words, a good washing to begin with, and an abundance of fresh air."

The first of these cases I regard as presenting a type of the average Hygienic character of hospitals in general, as *they are;* the last, a type of what they *should be*, in this respect, excepting, of course, the materials and style of structure. In making a few remarks upon them, I will ask attention to one fact, which contrasts them still further. Two of the most frequent troubles which the physician meets with in the treatment of typhus, under ordinary circumstances, are erysipelas and pneumonia, which supervene in no inconsiderable number of cases. It is very generally believed that the former complication is a more or less direct result of the impure air, causing it, oftentimes, to become endemic in a ward or hospital; while pneumonia, on the other hand, is more commonly attributed to exposure of the patient to a draught of air, in some way or another, even when the

manner or period of the exposure cannot be defined. In the last of the two cases I have presented, it does not appear that either of these complications occurred in a single instance, notwithstanding the unusual exposure of all the sick; while it is well known that in ordinary hospitals, as I have stated, both are frequently noticed.

In one of the shanties at the Ward's Island Emigrant Hospital, then occupied by typhus cases, it was once remarked to me, by the physician on duty, that pneumonia seemed to run from patient to patient along the whole length of the ward, a circumstance which he attributed to the cold air from the windows impinging upon the patients' heads, although no windows were open; but which, to my mind, was rather caused by a *want* of pure air, the atmosphere of the ward being exceedingly foul, and there being no way by which the external could find access. And I respectfully submit the question whether, if erysipelas, as undoubtedly is the case, is caused by the action of a foul and infected atmosphere upon the patient, be such action indirectly through his general system, or directly upon his external tegument, pneumonia may not be regarded as an erysipelas of the pulmonary tissues, produced in the same manner, seeing that they are exposed, both directly and indirectly, to the influence of the same foul atmosphere.

Regarded in its general aspect as a source of life and health, an ample supply of pure air, in conjunction with the immediate removal of secreted and exhaled impurities, beyond the possibility of re-inhalation, is a subject of profound interest to all humanity; but to the practitioner of medicine it presents itself with increased force. There is imposed upon us a double obligation. The question should be constantly before our minds, whether we shall deny, or allow to be denied, to our patients, the use of oxygen, in the fullest measure in which it can be found in the atmosphere? Whether, while searching our Materia Medica for the most

appropriate remedies, according to our theories of disease and treatment, we will continue to overlook the most potent of all restoratives, that derived from nature's own laboratory.

If we can believe and understand that by the influence of the rays of the sun upon its different aspects, the towering pile of granite on Bunker Hill is caused continually to sway to and fro upon its base,—with equal readiness may we comprehend that the refined and delicate living animal organism will vary in its phases of health, with the varying quality of the air upon which it depends, every moment, for its actual existence.

It was about the middle of the seventeenth century that Thomas Sydenham burst the trammels of prejudice, in which both the medical and the popular mind of his country, and of the world, had long been bound in reference to the innocuousness and availability of the operations of nature, and demonstrated the value, in the management of diseases, of the great medicament which she had furnished from the beginning of creation. When he tore away the bed-curtains, drove his patients from their sweltering beds, threw up their windows, or ordered them on horseback, the community thought him crazy; such kind of treatment was opposed to all their experience, and he had no authority for it from books. But, holding them in light estimation, when they contravened the obvious dictates of reason and nature, he consulted only the latter, and saved many from loathsome death by small-pox, and from premature graves by consumption.

One century later, the world was shocked by receiving from Calcutta, a horrible lesson of the consequences of confining human beings in a close and unventilated atmosphere. Ten hours sufficed to produce intolerable thirst, intense fever, delirium, and death, in one hundred and twenty-three out of one hundred and forty-six persons, and a high

putrid fever in those found alive at the end of that time. That "black hole" has ever since been a by-word and a reproach to humanity, while its lesson has been too little heeded.

And now, one century later still, and there comes from Perth Amboy, in the New World, a lesson of the omnipotent sanitary influence of that same subtle, invisible agent, —a lesson which should be treasured in the memory of all upon whom rests the responsibility of administering to the relief of their fellow-men.

Let it never be forgotten for a moment, that this agent, to procure which we have neither to dig into the earth, nor transport from foreign climes, nor distil from the alembic, nor refine in the crucible, but which is pressed upon us with a force, and in a measure, equalled only by the supreme benevolence which furnishes, and unceasingly renews it; this agent, when left free to act its part, removes the effete poison from the blood, and imbues it with continual health and freshness,—but when stifled and confined, whether intentionally or by accident, turns, like a viper, upon the arm that nourished it, and plants a deadly venom in its veins.

REPORT ON SOLIDIFIED MILK.

By the Standing Committee on Public Health and Legal Medicine.

read december 6, 1854.

The Committee on Public Health and Legal Medicine, to which was referred for examination and report, the subject of Solidified Milk, with a specimen thereof, Respectfully Report:—

In the increasing variety of circumstances in which man finds himself placed, in the progress of civilization and of art, there arise necessities for changes and improvements in his facilities for living, and indeed for the supply of his ordinary wants.

Thus, as population advances, muscle and the wind give place to steam for locomotion; the cotton gin and sewing machine supply the increasing demand for better and more abundant clothing; and the same progress is seen even in the luxuries and elegances of life; while food, the natural products of the soil, though vastly improved by the application of chemistry to agriculture, is among the last to yield to the demand for alteration of character. But as, in his adventures beyond the old limits of settlement, in his voya-

ges for discovery or trade, or in his long marches in numerous bodies into hostile countries, man is for months and years out of reach of the ordinary sources of supply, the necessity of concentration and durability in articles of diet becomes imperative.

It is in this aspect that every attempt to prepare food in such a manner as to preserve, at once, both its properties and integrity, its qualities of nutrition, and its incorruptibility, at the same time to reduce its bulk within portable limits, becomes a matter of great interest.

The earliest attempts on a large scale to preserve food of various kinds, so as to be available under circumstances in which fresh productions are unattainable, were about the year 1810. The antiseptic properties of salt, as applied to animal substances, it is true, were known from time immemorial, and the powers of sugar and acetic acid, when applied to the preservation of the finer and more delicate fruits, had doubtless been a long time recognised; but the objections to salt under many circumstances, and the tendency of the saccharine principle, in the mode in which it was usually employed, to undergo fermentation, rendered some other and different modes by which these objections could be avoided, a great desideratum. About the time mentioned (1810), which was not long after the discovery of oxygen gas, and the important *rôle* which it plays in the composition and decomposition of all vegetable and animal substances, an application of this important knowledge was made to the art of preserving food, and immediately there ensued a revolution as marked and extensive as that introduced by the daguerreotype in miniaturing.

The necessity of excluding the articles to be preserved from all contact with the air, soon became understood, and M. Appert, in France, suggested and put in practice, the plan now so well known, of preserving both vegetable and animal substances by confining them in hermetically sealed

glass vessels, which was soon after improved upon in England by the substitution of tinned iron for glass. In these preparations the addition of very little, if any, antiseptic material was found necessary; the entire process consisting in subjecting the food to a partial cooking, and then hermetically sealing the cans or bottles, after being perfectly filled, and all the air expelled by subjecting them to a high temperature in a water bath.

The discovery of this mode of preservation created no little sensation in the scientific world.

Another process by which some kinds of meats were, and are even now preserved, is by slightly salting, drying, and smoking; the desiccation requiring particular care. Under favorable circumstances this is an excellent mode of preservation. The article known as *Borden's Meat Biscuit*, heretofore reported upon by a committee of the Academy, is one in which the principle of desiccation alone is successfully followed. But there are many substances whose preservation is very desirable, which are composed in a very large degree of water, and which present a corresponding degree of difficulty in the efforts to preserve them.

Among these is the substance with whose examination your committee has been charged, as presented to us under a new form. As an article of food under all circumstances of life, civilized and barbarian, in infancy and in age, in sickness and in health, *Milk* is at once a luxury and a necessity; and it is remarkable that being such, it, of all the sources of man's sustenance, should be the most susceptible of spontaneous decomposition, and this not so much from the influence of the atmosphere, or any other external agent upon it, as from its own innate tendency, arising from its complicated and peculiar organization,—a few hours, and in warm weather even a few minutes, being sufficient to change its constituent character. Any method to preserve the integrity and nutritive qualities of milk, so that it may be

available at any time as diet, must therefore, if successful, be regarded as among the most interesting and valuable of the gifts of science to humanity.

We are not without information of attempts to preserve milk a long time for use, and it has undoubtedly been done so as to remain sweet and digestible, but as far as your committee have been able to ascertain, it has been by depriving it of a considerable portion of its nutritive principle, and adding to it some substances which, though innocuous as food, are nevertheless foreign to it and objectionable under many circumstances when pure milk is demanded.

The process of M. Appert (already mentioned as the originator of the improved food-preserving art) is described as follows:—

"Milk is to be boiled in an open pan, placed in the water bath, till it is reduced to half its quantity, *frequently skimming it,* and half an hour before it is taken out of the bath, it is to have the yolks of eight eggs added to a quantity of fifteen pints of the concentrated milk; which being put into bottles, they are to be boiled in the water bath for two hours."

In this manner we are assured that Milk may be preserved sweet a great length of time, but it is easy to see that the *long boiling and frequent skimming* must deprive it of no inconsiderable portion of its nutritive matter,—the substance which collects on the surface and is skimmed off, being principally casein, with more or less butter, whose loss must be very imperfectly supplied by the albumen of the eggs.*

To place the difficulty of preserving the nutritive consti-

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* An article designated as Preserved Milk, in the form of a paste, said to be manufactured in this city, a specimen of which was purchased by the Committee, is judged to have been manufactured by a process similar to that just quoted. Albumen evidently constitutes a considerable proportion of it.

tuents of this important fluid in its most striking light, let us take a glance at its chemical composition as presented to us by the most recent analysis. Cow's Milk contains both organic and inorganic constituents. The former are three, viz. *Casein*, *Butter*, and *Sugar of Milk*, equalling together 10·8 parts in 100. The inorganic substances are Phosphate of Lime, Phosphate of Magnesia, Phosphate of Iron, Chloride of Sodium, Chloride of Potassium, Soda; amounting together to a half of one per cent. In addition to these there is about 0·85 to 1.2 per cent. of Sulphur. The whole of these, except the butter, are dissolved in water, which constitutes about ninety per cent. of pure milk. Milk therefore is composed of about one-tenth organic matter, and one-half of one per cent. of inorganic salts, with a fraction less than nine-tenths of water. The *Butter*, being insoluble, is in a state of simple admixture, and being of less specific gravity, readily rises to the surface when at rest. It exists in the form of minute globules, of an average of 5000th of an inch in diameter. Each globule is formed of a film of insoluble casein, filled with a mixture of three different kinds of fat or oil, each of which is again composed of an organized substance known in chemistry as the *Oxide of Glycyrile* combined with some acid.

Casein is also a complex organic substance, and is chemically very nearly the same as albumen,—the difference principally being that albumen in solution will coagulate by mere heating, while casein will not. It may be obtained free from the other constituents of the milk, by allowing the butter or cream to rise, and removing it; then heating the residuum, and adding acetic acid, when the casein coagulates and is precipitated. Afterwards being placed on a cloth and all the water squeezed off, then well washed with water to separate the acetic acid, then with alcohol to remove all the butter, we have pure casein remaining, in the form of little white lumps.

It is then insoluble in water, and cannot be made soluble again by any known means. And even in ordinary fresh milk it is soluble only by its combination with soda, forming a natural compound which cannot be artificially obtained. The reason is now seen why the addition of an acid will coagulate it—the acid unites with the soda, which is removed from the casein, and the latter being set free, resumes its natural form of little white masses.

So, when taken into the stomach, the acids of the gastric juice produce the same effects. Milk always coagulates in that organ, and the casein, though insoluble, is digestible. Cheese is but insoluble casein, more or less mingled with the fluid parts of milk, and perhaps some butter.

The remaining organic substance is *Sugar of Milk.* This bears no essential difference in character from cane or grape sugar. It is this which gives milk its sweet taste. It may be procured from the whey of milk by evaporation. In the present consideration its interest lies in its great tendency to undergo fermentation, and become converted into lactic acid, which when once started cannot be arrested, and it progresses as long as there is any soda attached to the casein to be united with. There is, however, no lactic acid in fresh milk. We can thus account for the process of *souring* of milk, under ordinary circumstances, especially in warm weather, when it will sometimes occur in a few hours. The lactic acid derived from the fermentation of the milk sugar, unites with the soda of the casein, and sets the latter free.

From this brief review of the organic constituents of this important article, as well as from our universal experience with it, the obstacles to its preservation in such a manner as entirely to avoid any change in its elements, and to maintain their nutritive properties unimpaired, and without the addition of any other substance, might well be regarded as insurmountable. The two great enemies to the integrity of milk,

are time and caloric; either of which alone, under ordinary circumstances, will obtain the mastery and destroy it; but its destruction is rapid in proportion to the elevation of its temperature.

It is a matter of no little satisfaction, therefore, to your committee, to be able to state to the Academy their belief, derived from a very thorough examination of the whole subject, that the difficulties to the consummation of this most desirable object have at length been entirely overcome, and that henceforth we may, under all circumstances, in any position, and at any time, be supplied with pure milk, in a very convenient form.

The process by which these difficulties are overcome in the laboratory of Mr. S. T. Blatchford, is equally ingenious and simple. It has been fully subjected to our critical examination; we have traced the milk of the rich pasturage of Dutchess County, from the udder to its final conversion into the solid tablet; and we find it, in all its stages and appliances, to be based upon a thorough knowledge of the chemistry and dynamic tendencies of the natural fluid. It is not within our province, nor would it be proper here, to detail the steps of this operation; and it will suffice for this to state that the article called "*Solidified Milk*," obtained from that locality, and presented to us for examination, is nothing but the solid constituents of pure milk, combined with a little less than an equal part, by weight, of white sugar; that it contains no other foreign substance; that the various solids of the original fluid are preserved intact, even the butter globules being unbroken; that it is readily and perfectly soluble in water, and when it is so dissolved in proper proportion, it is in fact milk, as it was secreted by the cow, with the sole exception of the sugar which accompanies it; that the only medicinal or culinary operations in which ordinary milk is required, and this article cannot be used, are those in which sugar is inadmissible, while on the other

hand, whenever sugar is required in connexion with milk, they are here found together.

In fact, the article under consideration presents some advantages over ordinary milk, even of the purest quality, arising from the variety of degrees of solution in which it may be employed. For example, in some diseases of the digestive organs, whether of infantile or adult life, where a more concentrated nutriment than ordinary milk may be required, it is at once attainable, as this article may be administered either in a solid or liquid form; it may be eaten like candy, or dissolved in almost any proportion, and in its ordinary use at the table or in the kitchen, we at least have the advantage of being able to dilute it for ourselves, instead of being obliged to rely upon the uncertainty of the extent to which this process is carried by those who supply us at our doors.

The material relied upon as the preservative agent, is sugar, and that alone. The same principle is brought into use that is employed for the preservation of some of the more delicate chemicals, as the iodide of iron, towards which the saccharine matter acts as a preventive of decomposition, though in the case under consideration it has the advantage of being thoroughly dry, and thus avoids the possibility of decomposition as long as it is kept so—the syrup form being more prone to change. The very thorough admixture of the sugar with the solid matter of the milk, the whole being brought to a brick-like density, removes any danger of change as long as it is kept free from moisture.

Your committee have perhaps said all that is necessary to satisfy the Academy that the article before us is not only available as a *substitute* for milk, but that it is in fact *pure milk itself*, with the addition of sugar only. There is no loss of any nutritive material, a fact of which we can always be assured, for the article cannot be produced except from *fresh* milk, as any change in the character of the original fluid,

either by spontaneous decomposition, or otherwise, must spoil the result. So any dilution of the fluid by water must only lengthen the process of manufacture and thereby endanger the issue. On the other hand, the more abundant the nutritive ingredients of the milk, in other words the less the proportion of water, the more speedy and certain is the process, and the better the result. In an economical point of view, therefore, there is every inducement for the use of the purest material in the manufacture.

Your committee, therefore, not only regard it as a valuable addition to the resources of the voyager, the soldier, or any living beyond the reach of milk in its ordinary form, but we feel that we may congratulate the entire home community upon having always at hand an article which is free from the suspicions which, especially in cities, hang around the substance served to us under that name. At a time when public attention has been directed to the subject of milk through official and other reports, and the fact is disclosed that to a great extent, the so-called "pure milk" which is distributed to us daily, in scanty measure, is either partly manufactured from substances entirely foreign to it, or is the product of badly nourished and sickly city cows, to whom grass is an entire stranger, it is as refreshing to look upon the "solidified milk" now under consideration, the product of rich pastures and healthy cattle, as it is to breathe the pure air of their native hills, after a long exposure to the foul atmosphere of a city tenant house. The adulteration of milk so systematically pursued, has aroused the indignation of the community, but we have heretofore been helplessly at the mercy of those who, regardless alike of the premonitions of conscience and the health of families, appear to be actuated only by motives of cupidity. Perhaps no public grievance has ever met with a more complete remedy than may this one, by the introduction of the article under

review; it would seem capable of meeting fully all the hopes of the economist and the invalid.

"Blatchford's Solidified Milk" comes to us mainly in the form of a tablet, covered with tin-foil. It has a light yellow, slightly mottled appearance, is of very firm texture, but yields readily to the knife or grater. Each tablet weighs a pound, and will make in solution five pints of rich milk. When pulverized, it is readily soluble in hot or tepid water, while cold water requires rather longer time, but the solution is perfect in either. The tepid solution approaches nearer the standard of natural milk. The only objection, besides that of the presence of sugar, which can be made to it is an empyreumatic flavor somewhat similar to that of boiled milk, which it receives in the process of manufacture. This varies in degree, but is much less distinct when the solution is made with cold than with hot water, and in the preparation of custards, puddings, arrow-root, wine whey, ice creams, &c., in all of which your committee have practically tested it, it disappears. To many persons this flavor is not objectionable, and with many others, the palate soon becomes reconciled to it, and in a short time fails to recognise it. By contemplated improvements in the apparatus for manufacturing it, this objection is expected to be entirely obviated.

A solution in cold water, allowed to stand sufficiently long, will exhibit a surface of rich cream, much more abundant than is found in the same quantity of milk carefully selected in the city; from this cream your committee have caused good butter to be made, from which all traces of sugar are easily washed away, showing that this substance is only mechanically, and not chemically, incorporated with the milk constituents. In solution, it does not acidify so soon as the milk of city consumption.

Besides the hard tablet, the solidified milk is also furnished in a granular form, inclosed in cans; it is thus more con-

venient for use, and will doubtless keep sweet for many months, though probably more liable to change than in the other form.

With regard to the economy of its use for ordinary purposes in families, its cost (25 cents a pound) might appear to be a bar to its general introduction. But when it is considered that the milk thus made is much superior in quality to that commonly found in this city, and that the sugar it contains renders necessary so much less of that substance in its application to various uses, to say nothing of the satisfaction derived from the assurance of its purity and healthfulness, the cost is quite balanced. But as an article of diet in sickness, considerations of economy become insignificant in comparison with purity and high nutritive character, and your committee deem it but a duty to their fellow practitioners, to call their attention to it, as a valuable adjunct in the sick room, especially in the convalescence of children.

In conclusion, your committee have only further to remark in reference to its *durability*, though they have insufficient personal experience to speak from, that they are nevertheless satisfied from the representations made to them by responsible persons, that all that is claimed for it by the proprietors in this respect, is fully justified.

Respectfully submitted,

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CASES ILLUSTRATING

THE TREATMENT OF

BADLY-UNITED FRACTURES OF THE THIGH.

By Gurdon Buck, M. D.,

SURGEON TO THE NEW YORK HOSPITAL.

READ APRIL 4, 1855.

Case I. Jules Cæsar, seaman, native of France, age 19 years, fell from aloft on the 27th of March, 1849, and fractured his left thigh near the middle. On the 3d May, he was received into the New York Hospital, into Ward No. 7 of the 2d Surgical Division. Union had taken place—yet a slight degree of motion was still perceptible at the seat of fracture. The overlapping ends of the fragments bowed outwards, and were surrounded with considerable callous deposit. By careful measurement, the limb was found to be two inches and a quarter shorter than its fellow. On the 9th May, six weeks after the injury, the thigh was re-fractured in the following manner:

Jarvis' Adjuster was first applied, and the thigh put powerfully upon the stretch, as is done in reducing a luxation of the hip. The thigh being then seized at the knee and below the hip, was forcibly bent over the knee of the operator in every direction, so as to liberate the fractured ends, and disengage them from each other.

The rupturing of the adhesions was audible to all present. The patient had obstinately refused to be etherized; yet he did not complain of severe pain during or after the operation.

The limb could now be brought down to its full length with the hands alone; when left to itself, it resumed its original shortened condition. Without delay, the limb was bandaged and put up in the straight apparatus, and extension kept up as constantly as the patient could bear. At the end of three weeks, firm union had taken place, and the difference in length was reduced to less than one inch.

After another three weeks, the limb was transferred to the double inclined plane, and passive motion, together with frictions, employed to overcome the rigidity of the knee-joint, produced by the long confinement and inaction of the limb.

On the 17th Aug., fourteen weeks after the re-fracture, when the patient took his discharge, he could walk well with the aid of a cane, the shortening of the limb being still less than one inch. In treating this case, we had not yet the advantage of the improved mode of keeping up constant extension, which has since been brought into use in the hospital.

Case II. John McKee, laborer, native of Ireland, age 19 years, admitted into the New York Hospital on the 11th Oct., 1851, with fracture of the os femoris at its middle, caused by being struck with a fragment of rock from a blast.

An external wound over the seat of fracture, supposed to communicate with it, deterred from the employment of any active measures for keeping up extension.

On the 4th Dec., nearly eight weeks after the injury, it was found that partial union only had taken place. The limb was shortened two inches, and considerably bowed outwards at the seat of fracture. The external wound having closed, it was determined to re-fracture the limb, and subject it to constant extension by a new and improved method, which consisted in applying to either side of the limb, and in contact with the surface, a band of adhesive plaster of double thickness, three inches in breadth, and long enough to extend from the seat of fracture to ten inches below the foot. The lower ends of the bands being overlapped and stuck together, formed a loop, within which a square block of wood was placed. This block served as a point from which traction was

to be made. Over the bands a roller was to be applied in the usual way, from the toes upwards. This contrivance permitted constant extension to be kept up, without pain, or excoriation, or sloughing of the skin.

Its advantages in the treatment of primary fractures of the thigh had been fully tested in practice at the hospital, and its application to cases like the present, where so much depended on maintaining constant extension of the limb to the utmost degree, it was thought would be peculiarly advantageous.

Instead of Jarvis' Adjuster, the ordinary long splint was used in this case to put the limb upon the stretch.

As in the preceding case, the thigh, while on the stretch, was bent in every direction, till all the adhesions were ruptured, and the difference in length reduced to three-fourths of an inch. From the sensation perceived during the operation, it was evident that the uniting material was chiefly callus.

The new method of extension was forthwith resorted to, in connection with the straight apparatus.

At the end of eight weeks the apparatus was removed, and the fracture found firmly united, the shortening of the limb being less than an inch.

When discharged from the hospital, on the 27th April, patient walked about on crutches, and was gradually regaining the use of his limb.

Case III. On the 25th January, 1853, Richard Williams, a seaman, 21 years of age, was admitted to the hospital, with fracture of the right thigh, near the junction of its upper and middle third, that had happened by falling from aloft sixteen days prior to his admission. But little swelling remained about the seat of fracture. The shortening of the limb amounted to two inches and three-quarters.

The limb was put up in the straight apparatus, and constant extension by the new method employed, as in Case No. II., with the view of stretching the callus without rupturing it.

After the expiration of three weeks, it was found that a difference of two inches in the length of the limb still existed, notwithstanding extension had been kept up to the full degree. Five weeks having now passed since the injury, and

so little having been gained by stretching the callus, it was determined to rupture it, as in the preceding cases. This was done on the 14th February, with the aid of ether, and had the effect of reducing the difference in length to one inch. The previous treatment was at once resumed, and continued for thirty-one days. On removing the apparatus, firm union had taken place, with a shortening of scarcely one inch.

Case IV. John Healy, a seaman, native of England, aged 19 years, admitted on 17th February, 1854, into Lodge A. in the New York Hospital, with injuries that had happened eight weeks ago, in consequence of a fall from aloft, from a height of about sixty feet. The right thigh had been fractured near the middle, and united with great deformity of the limb. Short splints having been applied to the thigh by the mate of the vessel, and the limb below the knee left unsupported, the leg, with the lower fragment of the femur, had rotated outwards, and union had taken place in this position, so that the foot rested on its outer edge as the patient lay on his back with his limb extended. An exuberant deposit of callus surrounded the fractured ends, and no motion was perceptible at the seat of fracture. The absolute shortening of the limb could not be ascertained, for a reason that will appear in the sequel. Besides the fracture of the right thigh, the left hip had also sustained a serious injury. The limb lay powerless, and was rotated outwards. The trochanter major was drawn higher up towards the crest of the ilium than natural, as was evident from a comparison of the two sides. The limb could be rotated with equal facility inwards and outwards, and crepitus could be felt with the hand grasping the trochanter. Severe pain in the joint was produced by these manipulations. By extending the limb, it could be lengthened one inch, but, when left to itself, it resumed its original length, which was found, on measurement, to be one-fourth of an inch shorter than the right limb. The existence of a fracture through the neck of the femur, still ununited, being thus ascertained, it became a matter of the utmost importance to our unfortunate patient to rectify, if possible, the

distorted condition of the right limb; for it was found that he had no power when assisted to stand up, to bear any weight on his left leg, and he could only support himself on the right leg by throwing himself far over to his right side. It was therefore determined to re-fracture the right thigh, and treat it *de novo.*

This was done on the 20th February, with the aid of Jarvis' Adjuster, the patient being previously etherized.

Immediately after the operation, the limb was put up in the straight apparatus, and a moderate degree of extension only kept up. Special care, however, was taken to maintain the limb in its true bearings. The left limb was at the same time placed on the double inclined plane. After six weeks, the apparatus was removed from the right limb, and solid union found to have taken place, the position and bearings of the limb being natural.

One week later, the left limb was removed from the inclined plane, and, on examination, crepitus could no longer be felt at the hip, nor any elongation produced by extension, nor pain excited by rotation of the limb. The trochanter still appeared more salient and higher up on the pelvis than natural. The right leg was half an inch longer than the left.

At the time of his discharge from the hospital, the 5th July, the patient, who had steadily improved, was able to walk without crutches, supporting himself erect on both limbs alike.

Case V. John Land, a Norwegian seaman, aged 29 years, of a robust constitution, was admitted 19th May, 1854, into Lodge C., New York Hospital, with fracture of the lower jaw and left arm, besides a fracture of the left thigh at its middle. He had fallen from aloft at sea, a height of about forty feet. Though five weeks had passed since the accident, no bony union had taken place in either of the fractures. They were still very movable, though an exuberant deposit of callous material surrounded the ends of the fragments, both of the arm and thigh. The shortening of the thigh amounted to four inches, with extensive overlapping of the fragments at the seat of fracture. This deficiency of bony union was, no

doubt, to be attributed to a cachectic condition of the patient, induced by the privations and sufferings he had endured since the accident. Restricted by the condition of his jaw to the use of liquid nourishment, and only scantily supplied with such as was suitable, he had suffered much of the time from hunger; his body had become emaciated, and his complexion sallow and anemic.

Our first care was to renovate his impoverished system by an abundant supply of nutritious food, in a liquid form, and also by the administration of tonics. Soon after his admission, an extensive abscess developed itself at the middle of the left leg, on its outer surface, and was followed by sloughing of a considerable portion of the superficial muscles. This complication rendered it impossible to employ any means of reducing the amount of shortening. Eleven weeks more passed before the abscess healed and the limb regained a condition that would admit of any attempt to improve it. In the mean time, with restored health, bony union had taken place in the fractures, and the patient's deformity had become permanent for life, unless surgical art could interpose for his relief. Though sixteen weeks had now transpired since the original injury occurred, it was determined to re-fracture the femur, and endeavor, by constant extension, to reduce the amount of shortening. On the 6th August, the operation of re-fracturing was performed, the patient being first subjected to the influence of ether. While powerful extension was kept up by means of Jarvis' Adjuster, the thigh was flexed in every direction, and the union between the ends of the fragments broken up. The shortening was now reduced from four inches to one and a quarter.

The straight apparatus, with long extending bands of adhesive plaster, was applied to the limb, and special care taken to keep up the extension to the utmost degree during the subsequent treatment.

At the expiration of forty-four days, solid union had taken place, with a shortening of one inch and three-eighths.

The rigidity of the knee and ankle joints, and the thicken-

ing and swelling of the cellular tissue, consequent on the preceding inflammation and long continued inaction of the limb, very much retarded the restoration of its functions. He has, however, been gradually improving, and has the prospect of regaining, in a good degree, his former activity.

Case VI. Is that of a boy, five years old, still under treatment at the New York Hospital, by my colleague, Dr. T. M. Halsted, who has kindly permitted me to make use of the case.

He was admitted into Ward No. 2 of the 1st Surgical Division, on the 3d February, 1855, with a fracture of the left femur, that happened about four months previously, from a fall from off a front stoop. The limb was put up with a simple anterior and posterior splint, by a bone-setter. The result at the end of four weeks not being satisfactory, the limb was re-fractured, and again put up in the same manner. At the end of another four weeks, it had again united with deformity, and was re-fractured a second time. In the same manner, the limb was again re-fractured for the third time; after which the patient was sent to the hospital. When admitted, the limb presented a great degree of deformity. The upper fragment, which consisted of the superior third of the bone, overlapped the lower anteriorly and to the outside, with a very marked bowing outwards, and a shortening of two inches and three-quarters by accurate measurement. The union, though quite firm, permitted a slight degree of motion at the seat of fracture.

The limb was now put up in the straight apparatus, and constant extension applied, by means of adhesive bands, in the hope of stretching the callus. Counter pads were also adjusted near the seat of fracture to overcome the bowing. This plan was continued for twenty-seven days, till the 2d March, when it was found that the shortening had only been reduced to two inches. It was therefore decided, in consultation, to re-fracture the bone once more, and for the fourth time. This was done with the aid of ether by manipulation alone, and without stretching the thigh. The limb was again

put up as before in the straight apparatus, and, on applying extension, was brought down to within one-quarter of an inch of its natural length.

On the 31st March, after the treatment had been continued four weeks, the limb was examined, and union found solid, with a shortening not exceeding one-fourth of an inch.*

Remarks.—Some of the causes of deformed union after fracture, are illustrated by the above cases.

In the absence of surgical aid at sea, for example, the fracture remaining unreduced, the unresisted contraction of the muscles draws the ends of the fragments of bone past each other, producing a shortening of the limb, which, if allowed to continue, becomes permanent. The same result happens where the fracture is complicated by other injuries, or by coexisting diseased conditions of the limb itself, such as erysipelas, abscess, sloughing, &c., making it impossible for the surgeon to secure for the limb a proper condition, or apply the fixtures indispensable to maintain it in a state of reduction. The intractability of the patient, or a state of mania or delirium may also thwart the best-devised plan of treatment, and produce the same result. Removing the splints and apparatus from the fractured limb too soon, and premature attempts to use it, are also sure to produce deformity.

The deformity thus resulting may be the shortening of limb to a greater or less degree, without any deviation from its natural bearings. The minimum amount of shortening to be regarded as a deformity requiring or warranting surgical interference, must depend mainly on the inconvenience experienced by the patient himself. It can hardly be rated at less than one inch and a half, though with the present improved mode of keeping up constant extension by adhesive plaster, the attempt may be made under favorable circumstances to diminish even that amount of shortening.

Besides shortening of the limb, there may be bowing out-

* This patient, when discharged from the hospital on the 29th of May, was able to walk without crutches, the fractured limb being still only one-fourth of an inch shorter than its fellow.

wards or inwards at the seat of fracture. There may be also an outward rotation of the limb on its axis below the fracture. The effect of this is to change entirely the bearings of the limb, which is strikingly illustrated in case IV.

Accident must have early suggested to surgeons the mode of treating this class of deformities. Patients, in their first attempts to resume the use of a fractured thigh or leg, frequently re-fracture the newly united bone by accidental falls, and this is generally followed by a speedy re-consolidation of the fracture; sometimes this has proved an advantage, by enabling the surgeon to correct a defect in the original cure. This was illustrated in a recent case at the hospital. A young man, seventeen years of age, was treated during the summer of 1854 for a compound fracture of the left thigh near the middle. The wound complicating, the fracture became the seat of hospital gangrene, and prevented the application of any extension to the limb. The result was a shortening of one and a half inches at the date of his discharge on the 3d October. On the 25th of the same month, and nearly sixteen weeks after the first fracture, he was admitted again with a re-fracture, caused by an accidental fall. There being now no external wound to interfere with the treatment, constant extension was applied, and speedy re-consolidation effected, with only half an inch shortening.

When the interval since the fracture is not too long, and the ends of the fragments are still movable, extension alone may serve to elongate the limb sufficiently. If, however, after a few days' trial, no material advantage has been gained by stretching the callus, there should be no longer delay in rupturing it. The method of operating employed in the above cases, has proved safe, and it is believed it may be resorted to with very little risk of injury to the soft parts. By keeping the limb powerfully on the stretch while flexion is made, the ends of the fragments cannot be protruded far towards the surface in any direction. The inflammation and swelling following the operation, if any, are so slight as not to interfere with the immediate application of the apparatus for maintaining extension.

An important practical question in relation to this subject is, what interval of time may elapse within which it is admissible to resort to this mode of treatment? Guided solely by the consideration of the time required by the callus to become consolidated, we should be restricted in the application of our treatment to much narrower limits than clinical experience would impose. There is considerable discrepancy among authorities on this point, their views being influenced by the mode of treatment which they advocate. Some advise extension alone, with the view of stretching the callus, and discard altogether the operation of rupturing. Dupuytren, who advocated extension without rupture, fixed the sixtieth day as the ordinary limit, though he admitted that, in exceptional cases, it might be considerably exceeded. Æsterlen (cited by Laugier, *Thése sur les cals difformes*, &c., &c., Paris, 1841), an advocate of re-fracture, places the limits at six months to a year for adults, and at a longer period for children. Prof. Velpeau (*Médecine Opératoire*, 2d edition, T. 1, pp. 604, 605) divides deformities consequent on fracture into two classes: 1st. Where there is shortening alone of the limb, with overlapping of the fragments. 2d. Where there is angular deformity.

In the first case, he thinks that re-fracture should not be attempted beyond the third month, for fear of fracturing the bone elsewhere; or, in case of separating the fragments, their cicatrized extremities should fail to reünite after being brought to confront each other by extension.

Our Case No. V. is an example of successful re-fracture beyond the limits prescribed by Velpeau, and the subsequent rapid re-consolidation of the fragments shows that the fears of non-union are not always confirmed in practice.

There is still less ground for fear of non-union, if we consider that, after refracture of a callus of so long standing, it is seldom practicable, and never necessary, to employ extension to such a degree as to confront the ends of the fragments. If the amount of shortening be reduced to one inch, or even one inch and a half, there still remains sufficient overlapping of the newly ruptured surfaces to effect a union, and also to se-

cure to the patient a useful limb, with little or no halting in his gait.

In his second class of callus with angular deformity, Prof. Velpeau advises re-fracture of the callus at any period of its existence, however remote, and relates a case of an adult patient with an oblique fracture of the femur, that had united with a shortening of five inches, who, by an accidental fall, re-fractured the callus ten months afterwards, and recovered in two months, with only one inch shortening.

ON INJECTION

OF THE

BRONCHIAL TUBES,

AND

TUBERCULAR CAVITIES OF THE LUNGS.

By Horace Green, M. D., LL. D.

READ DEC. 6, 1854.

The subject which I propose, in this paper, to consider, and the anatomico-physiological questions involved in this discussion, are, I believe, of sufficient importance in practical medicine to claim the special attention of the members of the medical profession. It is a subject which has been discussed recently in the London and other foreign medical societies; and it has received, moreover, the attention of some of the foreign medical writers of distinction. It is therefore quite proper that a question belonging, as this does, more especially to American Medicine, should be brought before the profession of this country. But there is another reason why I have concluded to ask attention to the subject matter of this paper.

Having, by many observations, established the pathological fact that certain lesions, hitherto unnoticed by pathologists, are the cause of a very common and troublesome sympathetic affection, I prepared, several months ago, a paper on "Aphonia arising from organic lesions," which paper I thought to lay before the members of the New York Academy of Medicine. But, I confess, a recollection of my earlier experience

in advancing some peculiar doctrines in medicine, positively deterred me for the time from bringing my views before the members of the profession of my own city and country. Accordingly, at the request of a corresponding member of the London Medical Society, who to some extent had become acquainted with my views, I transmitted the paper to be read before the London Society. It was received and read, and, with the discussion it elicited, was published in many of the British journals. For this paper the thanks of that society and its fellowship were voted to its author.

Fully believing that I have made other important observations, if not discoveries, of a medico-chirurgical nature—discoveries which, I believe, will lead to important changes in the prophylaxis and treatment of pulmonary disease—I have deemed it to be my duty to bring these views, *first*, before a professional jury of my own city and country, giving to them the opportunity of proving or disproving, of receiving or rejecting, the propositions advanced.

It is well known that medication of the larynx and trachea, "after the manner of Trousseau and Belloc" (as the journals, and many physicians in America, have long been accustomed to say), has been practiced not only by myself, but by many other physicians, both in this country and in Europe, and with an amount of success in the treatment of some of the diseases of these organs that has been highly satisfactory; few, I believe, at the present time refusing to admit the practicability and utility of topical treatment in certain diseases affecting the upper portion of the air-passages. Indeed, for a long time, it has not been deemed necessary to prove, to the more intelligent portion of the profession, the possibility of medicating the larynx and trachea by topical remedies; for among those who have employed successfully this method of treating disease, and who have acknowledged its importance, are to be found the names of many of the most eminent practitioners of this and other countries. It has therefore been with many a matter of much surprise, that two of the most distinguished men in Europe,—one of them the hitherto-acknowledged pioneer in this treatment,—

should recently, as they have, denied emphatically the possibility of passing a sponge-armed probang into the larynx and trachea, even of the cadaver, much less into that of the living subject! I refer to Professor Erichsen, of London, and Professor Trousseau, of Paris.

In a valuable work recently published in London, on the "Science and Art of Surgery," by John Erichsen, Professor of Surgery in University College, and Surgeon to University College Hospital, and which has just been republished in Philadelphia, Professor Erichsen, after alluding in a favorable manner to what American surgeons have accomplished in the topical treatment of diseases of the throat, thus remarks: "Some of its advocates, however, not content with curing in this way disease that is visibly seated in the throat, pretend that the morbid action extends down the trachea into the bronchi, and that it is necessary to follow it in these situations. They accordingly speak of sponging and mopping out those parts of the air tube, and of applying the caustic solution to them, as if this were a proceeding that could be adopted with as little difficulty as passing the sponge into the nares. I cannot believe, however, that this practice, though commonly spoken about, and professedly employed, is ever in reality carried out. Any one acquainted with the physiology of the larynx knows how acutely sensitive it is, and how, in its normal state, it resists the introduction of any foreign body by the most violently spasmodic fits of coughing; and any surgeon who has seen the effects resulting from the fair and complete inhalation of a drop or two of a solution of the nitrate of silver, in a morbid and irritable condition of this tube, must feel sure that no sponge saturated with a solution of this caustic could ever have been thrust down between and beyond the vocal chords. But not only does physiology and ordinary experience tend to disprove the possibility of such a procedure, but repeated experiments, both on the living and on dead subjects, have led me to the conclusion that it is utterly impossible to pass a whalebone, whether curved or straight, armed with a sponge, beyond or even between the true vocal chords. I have frequently en-

deavored to do this in cases of cut-throat, in which, the air-tube having been laid open, the passage of the sponge could readily have been observed, and in which the facility of doing so would have been greatly increased by the larynx being no longer subservient to respiration; but in no one instance have I succeeded in passing the sponge so as to make it appear at the external wound. In the dead-house, also, I have repeatedly endeavored to pass the curved whalebone tipped with sponge into the trachea, but have never yet succeeded in doing so by employing those manipulations which could alone be practiced on the living subject, or indeed by any ordinary degree of force.

"From the repeated observations I have made upon this point, I have no hesitation in expressing my conviction that the sponge has never been passed, in the living subject, beyond the true vocal chords; though I believe it is possible that, in some instances, and with the requisite dexterity, it may for a moment be introduced between the lips of the glottis, so as to apply the solution freely to those parts; not, however, without inducing the most intense and spasmodically suffocating cough. I have little doubt that, in those cases in which the sponge has been supposed to have been passed between and beyond the vocal chords, and in which the operator speaks of having felt the constriction exercised by these in its entry and exit, it has, in reality, not entered the larynx at all, but has been passed behind this tube into the œsophagus, the constriction being produced by its passage beyond the projection of the thyroid and cricoid cartilages, and that the caustic solution has been applied to the mucous membrane in this situation, instead of to that lining the interior of the air-passages."*

Prof. Trousseau, of Paris, also, not only renounces in favor of Bretonneau all claim to priority in medicating the larynx, but in his cliniques he declares that the passage of

* The Science and Art of Surgery. By John Erichsen, Professor of Surgery in University College, and Surgeon to University College Hospital. London, 1853. pp. 694–5.

the sponge-probang into the larynx, either in the living or dead subject, *cannot be accomplished.* With regard to the former subject, I will give Prof. Trousseau's disclaimer, in his own words, contained in a letter to myself, received a few months ago. After alluding to another subject, Prof. Trousseau thus remarks:

"I begin by asserting that never, either before or since the publication of your labors, have I attempted to introduce into the larynx, or into the trachea, a sponge saturated with a caustic solution. I have done it several times *after tracheotomy*, but never without having previously performed that operation.

"Now, I shall endeavor to indicate the part that my preceptor, M. Brettonneau, and myself have taken in reference to *topical medication*, applied to diseases of the larynx.

"M. Brettonneau, as early as the year 1818, carried over the aryteno-epiglottic ligaments, several times a day, a sponge fastened to the extremity of a piece of whalebone, and charged either with pure chlorohydric acid or with a saturated solution of nitrate of silver. He expressed (the fluid from) the sponge at the entrance to the larynx, and the patient, in the convulsive movements of respiration, caused a certain quantity of the caustic solution to enter therein. He also caused vapors of chlorohydric gas to be inhaled. He did this in the treatment of croup.

"When he performed the operation of tracheotomy, in the last stage of croup, he carried, with a small sponge, a caustic solution into the trachea, and into the larynx, and penetrated into the trachea by the wound he had made. You will find all these details set forth in the 'Traité de la Diphthérite,' published at Paris, by M. Brettonneau, in 1826.

"M. Brettonneau, with an immense and deserved reputation, practiced, and still practices, at Tours, on a small field. I, his pupil, have been able, in Paris, to repeat his experiments on a far greater scale, and my labors in the treatment of croup have been published very often in the French and English journals. I need not at all refer to them in this

place; they were merely the practice of my old and illustrious master.

"But in 1830, I had, for the first time, occasion to treat chronic diseases of the larynx by caustic applications made to the superior portion of the organ, that is to say, to the aryteno-epiglottic ligaments. I made use, precisely, of the process which I have pointed out above in the treatment of croup, and I endeavored to express the caustic solution into the cavity of the larynx.

"The first two observations which I published on this mode of treatment can be found set forth in a well-known French journal, the *Bulletin de Therapeutique.* You will find them in the first volume, published in 1831, pages 163 and 276.

"I continued these labors on the diseases of the larynx up to 1835, the period at which Dr. Belloc and I sent to the Academy of Medicine a treatise on *laryngeal phthisis* and *chronic diseases of the larynx*—a work which obtained the grand prize the following year, and was afterwards published in the form with which you are acquainted.

"And, consequently, the direct introduction of the sponge, saturated with a caustic solution, into the larynx, and into the bronchial tubes, does not belong to me in any respect whatever; and even at the present time I content myself with expressing the caustic at the entrance of the larynx, or with causing powders or vapors to be inhaled.

"Accept, sir, and honorable confrere, the assurance of my high esteem. A. Trousseau,

"Professor of Medical Clinique of the Faculty of Medicine, of Paris."

Although Professor Trousseau here avers that he has never cauterized the larynx and trachea himself, except through the opening made in the operation for tracheotomy, yet he does not, in this communication, declare that the operation cannot be performed.

Since this letter was written, however, he has, at the Hôtel Dieu, in the presence of several medical gentlemen, expressed his conviction that the passage of a sponge-armed probang into the larynx cannot be effected.

The opinions of such men as Erichsen and Trousseau, on any question connected with medicine or surgery, must necessarily, I am fully aware, have great influence with the profession of both hemispheres. Their opinion on this subject, thus deliberately and emphatically given, is not to be ignored. It, therefore, seems to have become necessary either to relinquish our claim to this operation, or, by instituting a series of experiments, to prove, unanswerably, its easy and positive performance.

That great good may be accomplished by the topical application of a solution of nitrate of silver to these parts, when diseased, is not denied by these gentlemen. On the contrary, Professor Erichsen declares, that "the treatment of these various chronic inflammations of the pharynx and larynx requires to be conducted by the topical application of the nitrate of silver, which may be looked upon almost as a specific in these diseases;"* but he denies the possibility of applying the sponge to the parts below the vocal chords. On the other hand, we claim that this method of medicating the larynx and trachea is accomplished with much ease, and, ordinarily, with great certainty; and that, by this means, diseases of these organs, which would otherwise have proved mortal, have been repeatedly arrested.

We claim, and positively aver, what is susceptible of proof, that the sponge-probang, which Professor Erichsen declares, with as much positiveness, "has never been passed, in the living subject, beyond the true vocal chords," has been, in a thousand instances, "thrust down" between and beyond these chords, and has been carried not only through the trachea and its bifurcations, but, at different times, and in the presence of more than five hundred different medical men, has been passed, at will, into the right or left bronchial divisions!

I shall now give the proof, not only of the practicability of this operation, which has been so positively denied, but of our ability, as Americans, to accomplish still more than this

* Op. Citat., p. 694.

for the treatment of thoracic diseases; to perform operations of which the conservative Englishmen and skeptical Frenchmen have never dreamed!

When Dr. Marshall Hall came to this country, he held and expressed the same opinion that Erichsen now entertains with regard to this operation; for, when assured by Dr. Brainard of its practicability, he declared that "the passage of a sponge, wet with the caustic solution, into the larynx and trachea, as proposed, would prove fatal to animal life;" and yet, Dr. Hall was willing to see the attempt made, for he visited my office for this purpose, where he had an opportunity of witnessing its accomplishment in many instances; and it was he who suggested the employment of a tube, that the truth of its positive introduction into the larynx might be established, as it would be should the passage of the air be effected through the tube when in this position. After learning, therefore, these views of Professors Erichsen and Trousseau, I procured several of Hutchings' flexible tubes, of different sizes, and to the extremity of one of these, which is thirteen inches in length, I attached a sponge of the same size with those which are used with the ordinary throat probang.

At this time I had under treatment many patients having disease of the air-passages, into whose larynges and trachea I had passed (as I believed) the sponge-armed probang in many instances. But, as other proof besides my own opinion and belief was necessary to establish this in the estimation of many, I selected one of these patients, an intelligent clergyman, from Canada, and, wetting the sponge at the extremity of the flexible tube, in a strong solution of nitrate of silver, I passed it down to the vocal chords, through the rima-glottidis (as I supposed), and several inches into the trachea; then, withdrawing the wire from the tube, I directed the patient to close his lips and blow, and breathe through the tube. This he did for several moments, filling and emptying the chest of air repeatedly. A lighted lamp was then brought, and this he extinguished promptly, several times, by *blowing through*

the tube! This experiment was performed on the 5th of October, in the presence of several physicians.

But as it was intimated by one of the medical men present that it might be averred, by those skeptical on this subject, that the light was extinguished by the air passing through the nostrils, or by the side of the tube, another patient was selected for repeating the experiment. This gentleman was the Rev. Mr. McAnn, the superior third of whose epiglottis could be seen easily by depressing the tongue. Upon the laryngeal face of this cartilage, the extremity of the tube was placed and introduced readily through the chink of the glottis into the trachea. This operation was also performed in the presence of several physicians, among whom were Dr. Sims, Prof. E. H. Parker, and several others. The instrument being thus introduced, one of the physicians closed the anterior nares of the patient, and the light was again extinguished by the expired air through the tube. A large pasteboard card, perforated in the center, and of sufficient size to screen the nose and mouth completely, was then slipped over the tube, to which it fitted closely, and the patient directed to blow out the light, which was accomplished through the tube as promptly as in the first instance. After all this, Mr. McAnn was requested to expand the chest, by breathing through the instrument. This was several times performed—the patient inhaling and exhaling easily and freely through the inserted tube. These experiments were subsequently repeated on some eight or ten patients, always with the same results, and in each instance in the presence of several medical men. Among those physicians who were present on one occasion or another, I may mention the names of my colleagues, Professors Barker, Davis, Peaslee, and Parker, Dr. Bowditch of Boston, Professor Davis of the University of Virginia, Crawcour of New Orleans, Smith of Galveston, formerly surgeon in the army of Mexico, Rose of Indiana, Patterson of Ohio, Sims, Sayre, and Minor, of New York, and more than forty other physicians of this and other cities, every one of whom expressed himself satisfied with the success of the experiments, as proving the introduction of the instrument "between and beyond the true

vocal chords" into the trachea of the patients. In order to test, still further, the truth of the operation, a small, air-tight, elastic bag was tied over the upper extremity of the tube, and on introducing the instrument six or eight inches into the trachea of a gentleman, this little bag was inflated and collapsed a dozen times, by the acts of inspiration and expiration on the part of the patient. In performing this last experiment, an incident occurred (which, had the tube been shorter, might have proved an accident), that is an additional proof of the position of the instrument. The tube, which, as I have stated, is thirteen inches long, was introduced its whole length, so that the upper extremity was flush with the lips of the patient, the elastic bag, which is three inches long, only remaining out of his mouth. After the patient had filled and emptied the sac several times, I let go, for a moment, my thumb and finger hold of the extremity of the tube. Just then the patient made a strong inspiration, when the whole instrument, sac and all, was drawn suddenly in, and for a moment disappeared out of sight. Thrusting my fingers immediately into the throat of the patient, I could barely reach, at the base of the tongue, the upper extremity of the bag, which I seized with my thumb and finger, and drew the whole out together.

Other experiments, confirmatory of the above, were instituted, such, for example, as the suspension by a thread, of a small ball of floss before the mouth of a tube, which was alternately drawn into and expelled from the opening, by the act of respiration.

The above series of experiments were considered conclusive by those who observed them, in proving that the operation of passing the sponge-probang into the larynx and trachea, as we have claimed, is positively being accomplished; and we submit to the members of the profession, whether the dogmatic assertions of Professor Erichsen, that "the sponge has never been passed, in the living subject, beyond the true vocal chords," and that the operation "is utterly impossible," has not been by these experiments disproved?

But we have declared it to be possible to do still more for

the treatment of thoracic disease, than can be effected by this operation, though its practicability be fully admitted. After accomplishing what has already been described—namely, that of introducing the elastic tube into the bronchial divisions, for it must have passed several inches into these, if it entered the trachea—these questions occurred to my mind: What shall now hinder the introduction of medicinal agents, through this tube, into the lungs, or directly into the bronchi and their terminations? What will prevent the injecting, even of a vomica, under favorable circumstances, with appropriate remedies?

Having under treatment, daily, patients laboring under, not only chronic bronchial disease, but those affected with tuberculosis in almost every stage of the disorder, I determined to test the effect of a solution of nitrate of silver, applied directly and freely to the bronchi in disease of their membrane; also in disease of the lungs, to inject, if possible, the same solution into tubercular excavations.

The first trial of this nature was made on the 13th of Oct. 1854. It is unnecessary to give the previous history of this case, in which the operations of *catheterism of the air-passages* was first performed.

The patient, a lady from Connecticut, thirty-two years of age, is in the advanced stage of tubercular consumption; a large cavity exists in the apex of the left lung, and a deposition of tubercles is present in the right. All the physical signs of both these conditions are present; my own opinion has been confirmed by the examination and opinions of several good auscultators. She has had, for several weeks past, the treatment, both topical and general, ordinarily employed in the management of such cases. Once in two or three days, the larynx and trachea have been cauterized, and the iodide of potassium, with both vegetable and mineral tonics, have been administered, and with considerable benefit. But still, the signs of advanced tuberculosis are present. The cough and free expectoration of purulent matter continue.

Oct. 13th.—To-day, instead of using the sponge-probang, I passed No. 12 of Hutchings' elastic tubes (which is thirteen

inches long) through the trachea, and into the left bronchial division. Through this tube, with a small glass syringe, I injected one drachm of a solution of nitrate of silver, of the strength of forty grains to the ounce of water, into the lung. No cough whatever, or any sense of suffocation, was produced by this operation, nor did the patient observe in the least the ordinary bitter taste of the solution. A few minutes after the operation she stated that she "felt a warm sensation" in the upper portion of the left lung; but no pain, or any unpleasant feeling whatever, followed the operation. Mrs. A. did not return to have the operation repeated until the 17th, four days afterwards, when she stated that for twenty-four hours after the use of the injecting tube, her cough and expectoration were both greatly diminished, that she had breathed with more freedom than before; that these favorable symptoms had continued, though not as marked as at first, up to the present time. She was therefore much disposed to have the operation repeated. The tube was again introduced through the trachea its entire length, and at this time one and a half fluid drachms of the solution were thrown into the lungs. The immediate results were the same as at first; but, after some minutes, she began to cough, and expectorated easily, and at once, nearly two ounces of purulent matter, changed in its color and consistence, apparently, by its immediate contact with the argentine solution. Indeed, the expectorated matter presented precisely the appearance which is observed to take place with the purulent matter of an external ulcer when cauterized with the nitrate of silver. This changed condition of the expectoration was observed by several physicians who were present when the operation was performed. The relief which followed this last operation in Mrs. A.'s case was still more marked and decided than in the first instance. Her cough, she stated, was much relieved, the expectoration yet more diminished, and her breathing was easier. A pain in the chest, of which she had complained, was removed; and during the two nights which followed the operation, her sleep was better than it had been for a long period before. Mrs. A. remained until the 26th, during

which time the elastic tube was introduced into the left bronchial division seven times, and on each occasion from one and a half to two drachms of a strong solution of the nitrate of silver were injected into the lungs. Her improvement was constant. She grew stronger, and gained flesh in this period; but, being obliged at this time to return to her home, she left with the intention of coming back to renew the treatment, in a few weeks.

The same day on which I succeeded in introducing medication into the air-tubes of the above patient, I commenced in like manner the treatment of other cases, and since the thirteenth day of October, there have been treated for a longer or a shorter period, thirty-two patients laboring under tubercular or bronchial diseases, by the direct introduction into the lungs of a strong solution of the nitrate of silver injected through the elastic tube. Of these thirty-two cases, nineteen showed unequivocal physical signs and symptoms of tuberculosis in the different stages of the disease; complicated, many of them, with bronchial inflammation. Thirteen of the number are cases of chronic bronchitis, the disease in some of them being of many years' standing. Of the nineteen cases of tuberculosis, nine of the number presented, on auscultation, the usual signs of the presence of tubercular cavities in one or both lungs. All these cases of thoracic disease, with one or two exceptions, appear to be benefited, some of them greatly, by this method of topical treatment.

Although a rigid and circumstantial history of these cases, and of their treatment, is being kept by my assistant, Dr. Richards, I shall not detain the reader with a relation of them, but will merely select a few whose history and sanatory condition, on coming under my treatment, were known to other medical men; for all of these cases to which I shall refer were committed to my care by their attending physicians.

The first case of which I shall speak is that of Mrs. A., whom I first saw early in January, 1854, in consultation with a distinguished physician of this city, Dr. John W. Francis. She had suffered from chronic bronchial disease for several

years; but her symptoms, from taking cold, had been greatly aggravated some few months before this consultation, and on examination at this time, there were revealed signs of extensive bronchial disease, with tubercular deposition in the apex of the right lung. Marked dullness, on percussion, was found under the clavicle on the right side, with crepitating mucous râles and prolonged expiration, indicating the commencement of tubercular softening; on the left side puerile respiration, with coarse mucous râles over the whole chest. The disease seemed to have been preceded, or attended, by follicular disease of the pharynx; for the mucous crypts of the pharyngeal membrane were destroyed, and the right tonsil had become completely atrophied. Mrs. A. was feeble and much emaciated, had a severe cough with large muco-purulent expectoration. After continuing the treatment of the patient for some time, in consultation with her attending physician, at his request I consented to take charge of the case, on condition that the patient should visit me at my rooms.

In addition to the ordinary general treatment, which it is not necessary to particularize, topical applications of the nitrate of silver to the pharyngo-largyngeal membrane were employed. These cauterizations were continued, at first three or four times a week, and subsequently twice a week, during the remainder of the winter and the following spring. No diminution of the cough or expectoration occurred until the local treatment had been continued for several weeks. Gradually both improved; Mrs. A. gained in strength and flesh, so that when she left the city in June, to spend the warm season in the country, she had gained several pounds in weight, and, although the cough and expectoration still continued, both were greatly diminished. No marked change in the patient's symptoms occurred during the summer; but soon after her return to the city, at the close of the season, her pulmonary symptoms, from taking cold, or from some other cause, were aggravated, and her cough and expectoration were again increased. The treatment was renewed. Applications of the nitrate of silver were made to the larynx and trachea, which at once diminished, as before, the urgency of the symptoms.

On the 25th of October, I introduced the flexible tube through the trachea, and conveying it down the right bronchial division, injected one and a half drachms of the argentine solution, of the strength of forty grains to the ounce of water, into that side. As in the first instance described, the irritation was much less than when the sponge-probang is used. Indeed, no cough or disturbance in any degree followed this operation. The effect on the prominent symptoms of the patient was very favorable. For twenty-four hours afterwards, the cough and expectoration were greatly diminished; nor did tightness of the chest, or any uneasiness whatever, follow the suppression of the discharge. On the contrary, the patient experienced so much relief during the subsequent week, that she did not return until the 1st of November (one week afterwards) to have the operation repeated. On this day two drachms of the fluid were introduced; and thrice since, making five times in all, has the same operation been performed. Mrs. A. has continued constantly to improve, and notwithstanding the unfavorable season of the year, has certainly gained more in the last six weeks, than during several months before. But what the ultimate result will be, it is of course impossible to pronounce. Of one thing we are positive, that Mrs. A. is in much better health now than she was nearly a twelvemonth ago, at which time she exhibited unmistakable signs of tubercular exudation, complicated with general bronchial disease.

Some months ago, General P., of Niagara, having just returned from Europe, called on me with an invalid sister, bringing with them a letter from Professor Trousseau, of Paris, under whose treatment Miss P. had been, containing his full opinion of her case, and commending her to my care. The following is an abstract of the letter of Trousseau: "Miss P., whom I have examined with great attention, will hand you the consultation which I have given her. I think she has pulmonary emphysema, with chronic bronchitis; but I have not been able to find any of the signs of tubercular affection."

An attentive examination of this lady's case confirmed the opinion given by M. Trousseau. Extensive bronchial disease

existed, with pulmonary emphysema—a complication which, I believe, is almost always more or less present in serious and prolonged bronchitis.

No treatment was adopted at this time, as Miss P. was on her way home, but she proposed to return in a few weeks, and have the treatment I had advised employed. But I saw nothing more of this lady until the 15th of last October, when her brother came with her to New York, and placed her under my care. Her case now presented symptoms more unfavorable than when I first saw her. She was emaciated and feeble; was harassed by an incessant cough, and had a most profuse muco-purulent expectoration. Auscultation now revealed, not only extensive chronic bronchitis, and pulmonary emphysema, but bronchial dilatation. In addition to an almost constant bronchial cough, she was subject, daily, to paroxysmal attacks of spasmodic cough, so severe, at times, as nearly to suffocate her. The local treatment, conjoined with appropriate general remedies, was adopted in Miss P.'s case. The applications of a solution of nitrate of silver were made daily, first to the pharynx, and then into the larynx and trachea. Improvement, in her case, began, as it almost always does in bronchial disease, as soon as the small amount of caustic fluid which the sponge will take up, had been introduced a few times into the larynx and trachea. On the 4th of November I employed, for the first time, in her case, the tube and syringe, and injected, on this occasion, two drachms of the caustic solution into the bronchial divisions. The effect of this irrigation of the pulmonary mucous membrane, was, as the patient herself remarked, "perfectly astonishing." The cough and expectoration were both greatly relieved; the oppressive sensation about the chest, long complained of by the patient, was abated; and on her return to the office, three days after, she manifested improvement in every symptom.

The injections have been repeated every day and every other day up to the present time, and Miss P.'s improvement has been going on constantly. She is now much stronger, coughs less, has gained flesh in the last three weeks, and has exchanged her pale, sickly look, for one indicative of return-

ing health. To day, November the 28th, I injected, for the sixteenth time in her case, in the presence of Professor Davis, of the University of Virginia, Dr. Minor, of Brooklyn, and several other medical gentlemen, two drachms of a solution of the nitrate, of the strength of two scruples of the salt to the ounce of water, without producing the slightest cough, or any irritation whatever.

Of the thirty-two instances mentioned, of tubercular and bronchial diseases, it would not be difficult to select a dozen cases, from among those who have been the longest under this form of treatment, which have manifested signs of improvement as extensive and decided as have those cases to which we have referred. I shall, however, only allude briefly to one of these, the character and treatment of which has been observed, with much interest, by several medical gentlemen.

On the 21st of September, Dr. Varick, of Poughkeepsie, placed under my care a young lady from that town, who for several years had been afflicted with chronic bronchitis of a grave character. The disease began four years ago; commencing in the form of a follicular inflammation of the pharyngeal membrane, and extended by continuity to the mucous membrane of the larynx and bronchi. The disease was attended with a loss of voice, an almost constant and harassing cough, and a profuse muco-purulent expectoration. Miss V—— presented many of the rational signs of tubercular consumption; but auscultation revealed only mucous and bronchial râles over both lungs, with vesicular emphysema and bronchial dilatation. There were no positive signs of the presence of tubercular exudation, although slight flatness, with prolonged expiration existed on the right side. Dr. Varick had employed, in his treatment of her case, along with general remedies, the topical application of the nitrate of silver; but as he had not been able to medicate the larynx, and the patient was not relieved, he requested me to take charge of the case. I soon succeeded in cauterizing the larynx and trachea, both of which were ulcerated; and after continuing the local treatment for two or three weeks, the voice was restored to nearly its normal condition, but the cough was not

materially relieved, nor was the profuse bronchial expectoration in any degree diminished. A deep seated, dull pain under the sternum, which had long continued, and which had resisted the effects of counter-irritation, and other measures, still remained.

On the 3d of November, in the presence of several medical gentlemen, I passed the tube through the larynx and trachea, down to the bifurcation, and injected into the bronchial divisions, two drachms of a strong solution of the nitrate of silver. From this hour, her troublesome symptoms began to improve, the cough which heretofore had vexed her night and day was arrested completely, for a period of twenty-four hours after this first operation; and what was equally surprising, the pain under the sternum, which had been so persistent, ceased altogether, and has not since returned.

On the second day, she coughed again, but moderately, and expectorated with more ease than before. The injection was repeated on the 6th of November, and again on the 8th, and was attended, apparently, with continued advantage to the patient. The effect of the remedy was so prompt and decided in checking the profuse expectoration, that some fears were entertained by me, in this case, as well as in that of Miss P——, that unfavorable symptoms might follow the sudden suppression of such long-continued and habitual drains. But in neither of these cases, nor in any of those similarly affected and treated, have I observed any dyspnœa or oppression of the chest whatever, to follow the diminished expectoration.

Inasmuch, therefore, as the effect of the treatment in Miss V.'s case continued to be decidedly favorable, the operations were repeated every few days, throughout the month of November. During this time, she had added about ten pounds to her weight; her cough was nearly gone, her voice restored to its normal condition; and, as her whole appearance and symptoms were indicative of returning health, she was, on the first day of December, dismissed from further topical treatment.

In the treatment of the various chronic inflammations of the bronchi, I have been accustomed, for many years, to de-

pend upon the topical application of a solution of nitrate of silver to the mucous membranes of these parts; and so uniformly has success followed its employment, that, as Erichsen has said of its effects on the pharyngo-laryngeal surface, I can affirm, that in my hands it has proved almost a specific in these diseases. Others who have given the remedy a fair trial, have borne the same testimony in its favor. Dr. Allison, of London, who has published an interesting brochure on the "Medication of the Larynx and Trachea," says, "In chronic inflammation of the larynx and of the upper portion of the trachea, the solution of the nitrate of silver has, in my hands, as in others, been very useful in bringing the disorder to a conclusion; and where that has not been accomplished by reason of its dependence upon incurable disease of the lungs, it has almost invariably afforded very considerable relief, by rendering the cough less frequent and violent, and removing much of the tickling and uneasy sensations at the upper portion of the larynx. In this form of disease the treatment is particularly suitable, whether it be simple or of a specific character. * * * * *

"In some cases of disease of the larynx and trachea, in which the symptoms inclined to the suspicion that ulceration existed, the same local application of nitrate of silver has been very useful. Old bronchial affections have in a large majority of cases been similarly benefited."*

Professor Bennett, of Edinburgh, in his recent work on Tuberculosis, thus speaks of the effects of this local remedy: "The action of the nitrate of silver solution is not that of a stimulant, but rather that of a calmative or sedative. It acts chemically on the mucus, pus, or other albuminous fluids it comes in contact with, throws down a copious white precipitate, in the form of a molecular membrane, which defends for a time the tender mucous surface or irritable ulcer, and leaves the passage free for the acts of respiration. Hence the feeling of relief almost always occasioned; that diminution of

* The Medication of the Larynx and Trachea. By Scott Allison, M. D., pp. 7–8. London, &c.

irritability in the parts, which is so favorable to cure, and why it is that strong solutions of the salt are more efficacious than weak ones.

"It may be easily conceived that such good effects must be more or less advantageous in almost all the diseases that affect parts so sensitive, from whatever cause they may arise; and that this treatment is not adapted to one or more diseases of the larynx, but, like all important remedies, meets a general indication which the judicious practitioner will know how to avail himself of."* And he closes a work of great interest with the following "practical conclusions."

"1st. That not unfrequently diseases, entirely seated in the larynx or pharynx are mistaken for pulmonary tuberculosis.

"2d. That even when pulmonary tuberculosis exists, many of the urgent symptoms are not so much owing to disease in the lung as to the pharyngeal and laryngeal complications.

"3rd. That a local treatment may not only remove or alleviate these complications, but that in conjunction with general remedies, it tends in a marked manner to induce arrestment of the pulmonary disease."†

Hitherto, in the treatment of bronchial disease, a difficulty has arisen from our inability to introduce, by means of the sponge-probang, a sufficient quantity of the caustic solution into the bronchial divisions; for in passing the instrument into the opening of the glottis, and through the rima of the glottis, much of the fluid is discharged from the sponge before it reaches the tracheal division. On this account I have, in a multitude of instances, when treating bronchial disease, introduced the saturated sponge, several times, at the same sitting, in order to convey an increased amount of the fluid into the bronchi, and it has frequently happened, that patients observing its effects, have returned in a few days requesting

* The Pathology and Treatment of Tuberculosis. By John Hughes Bennett, M. D., &c. p. 140.

† Ut supra. p. 142.

that as much of the solution as possible be passed into the wind-pipe, as the cough and expectoration, they have declared, are invariably greatly improved by the operation.

By this method of catheterism of the larynx and trachea, the solution is not only conveyed with more certainty and directness to the bronchial mucous surfaces, but any amount of the medicament may, in this way, be introduced. What quantity of the solution may be the appropriate amount to be employed, in any given case; of what strength; how often to be repeated; or how long to be continued—are important questions, to be solved by future experience, and by repeated observations.

Of one interesting fact we are now fully assured, that whenever the remedy has been freely employed in the treatment of bronchial disease, the effects have been invariably salutary. Catheterism of the larynx and trachea has now been employed in my hands, in the treatment of more than twenty cases of chronic bronchitis—some of them of a very severe and protracted nature, in which from one to three drachms of a solution of nitrate of silver, of the strength of from thirty to forty grains to the ounce, have been injected every few days, in each case, through the trachea, into the bronchi; and in every instance, with not a single exception, improvement has followed the treatment. In those cases where tubercles exist, whether the exudation be in a crude state, or beginning to soften, the beneficial effects of the treatment have been, thus far, as uniform and certain, although the improvement has not been as rapid in these, as in the former cases. Most of these cases of tubercular disease are still under treatment, and the final result cannot be foretold.

In the employment of catheterism of the bronchi, by means of the flexible tube and the syringe, repeated proofs have occurred of the presence of this tube in the trachea. The coughing of the patient before the injecting was completed, has often driven a portion of the solution, with force, through the tube, and to a distance of several feet from the patient. Several distinguished auscultators, who have been present when the operation was performed, have had the

curiosity to examine the chest of the patients, both immediately before and after the operation, when they have detected readily, by auscultation, the presence of the fluid in the lungs, immediately after the injection was made.

Of more than fifty medical men, comprising many distinguished physicians of our country, who, from time to time, have been present at these operations, all, *with one or two exceptions*, have been fully satisfied that these injections were, in reality, made, not into the œsophageal tube, but into the tracheal and bronchial divisions.

Among the patients who have been the subjects of this treatment, is Dr. Pittard, an intelligent and experienced physician from North Carolina, who came to this city to be treated for a severe and long-continued bronchial disease. The following is Dr. Pittard's testimony on this subject:

"In compliance with your request, I give you a simple statement of the effect of an injection into my lungs, of the nitrate of silver. The application of the remedy caused a considerable glow through the chest, which was felt for several hours. There was no irritation produced on the bronchial membrane, by the introduction of the fluid; but, on the contrary, the cough was suspended, or greatly moderated for a day or two.

"It may be said that the injection passed into the stomach, instead of the lungs. This may have been possible in some other instances, but in my case there could be no doubt of its having entered the air-passages, for you will recollect that the breath was passed out through the tube, as soon as it was inserted, which may be considered conclusive evidence, besides, I tasted the nitrate of silver, in the matter expectorated, for twelve or fifteen hours after the administration of the remedy.

"Very respectfully,

"Jno. Pittard, M. D."

In conclusion, it is here maintained, that the direct medication of the lungs, by means of catheterism of the air-tubes, an operation, I believe, not before performed, has been re-

peatedly accomplished—that the operation may be performed by the dextrous surgeon with ease and facility, and with perfect safety to the patient, and that the results of this method of treating disease, whether it has been employed in bronchial affections, or in the commencement of tuberculosis, have already afforded the most gratifying indications that practical medicine will be greatly advanced by this discovery.

MAJORITY REPORT

OF THE

SPECIAL COMMITTEE,

TO WHICH THE PAPER OF DR. HORACE GREEN, ON "INJECTIONS INTO THE BRONCHIAL TUBES, AND INTO TUBERCULAR CAVITIES OF THE LUNGS," WAS REFERRED.

READ JUNE 20, 1855.

The Committee to whom was referred the paper of Dr. Horace Green, "On the employment of Injections into the Bronchial Tubes and into Tubercular Cavities of the Lungs," beg leave to Report:—

That they have given this subject that serious attention which its importance demands, and respectfully submit to the Academy the results of their investigations.

At the preliminary meeting of the committee, held on the 8th of January, it was determined,

1st. To settle the question of the passage of the instrument into the air-passages;

2d. To ascertain, as far as possible, the utility of injections of nitrate of silver into these passages.

The committee met on five different occasions: twice, viz., on the 13th of January and on the 1st of February, at the office of Prof. Green; and three times, viz., on the 20th and 27th of January, and on the 5th of June, at Bellevue Hospital. At these different meetings, thirty-eight patients, in the aggregate, were submitted by Dr. Green and others to the test of the operation of passing instruments into the air-passages, and three to that of the injection of nitrate of silver into these air-passages.

The details of these experiments will be found appended to this Report, of which they are the basis.

1st. As to the practicability of the operation of passing instruments into the air-passages.

This division of the Report may be considered under the following heads:

1. The history of catheterism of the air-passages.
2. The results of experiments with instruments.
3. The facility with which the operation may be performed.

1st. History of catheterism of the air-passages.

The introduction of tubes into the air-passages is by no means a recent operation. It was recommended by Hippocrates in severe angina where suffocation was impending, and seems to have been long practised in preference to bronchotomy. Desault, among modern writers, also recommended it strongly. Mr. Ryland, in his treatise on Diseases of the Larynx, devotes a short section to this operation, and discusses familiarly its practicability and utility. Other authors, whom we need not mention, allude to it as a matter of history.

2d. Results of experiments.

The results of the series of experiments, instituted by your committee, confirm the records of history, as to the practicability of the operation of catheterizing the air-passages. In eleven cases, it was performed to their entire satisfaction. The evidences on which they rely as proofs of its success, are:

1st. The detection of the tube in the cavity of the larynx by the finger passed into the mouth. This was done in some cases by several of the committee, and was demonstrative of the position of the tube in relation to the laryngeal opening.

2d. The symptoms manifested by the patient. The symptoms which mark the entrance of the instrument into the larynx, and its progress along the air-passages are unequivocal, and need to be witnessed in but a single instance to be recognized at once in every subsequent trial. They are, indeed, such as we ordinarily witness when foreign bodies enter these passages, and reasoning, *à priori*, such as we would naturally expect would be present. The instant that the tube enters the larynx, the patient is seized with a violent, spasmodic cough, the face becomes red and anxious, and the patient manifests an eager disposition to withdraw the instru-

ment or escape from the hands of the operator. While the tube enters the rima glottidis, these symptoms reach the maximum of their intensity; from being suffused and anxious, the face becomes swollen and livid; the veins of the neck and face turgid; the eyes wild, prominent, and overflowing with tears; the respiration, for the moment interrupted, is renewed with long, loud, croupy inspirations and expirations, attended with violent spasmodic cough and forcible ejections of bronchial mucus from the tube. So overpowering is the sense of suffocation at this moment, that the patient is with difficulty persuaded to allow the tube to remain, or to let his attention be diverted to other objects. If by assurance that the operation is complete, and that his sufferings will not be increased by the presence of the instrument in his throat, he can be quieted, the cough and sense of suffocation, in some measure, subside, and an opportunity is offered of testing the power of speech. In every instance, where the patient was induced to attempt to speak under these circumstances, the only voice emitted was a hoarse, croupy sound, which could, with difficulty, be construed into the simple words "*Yes*" or "*No.*"

In striking contrast was the train of symptoms developed on passing the instrument into the œsophagus. Retching and cough usually followed its first introduction into the fauces, and as it penetrated more deeply, vomiting sometimes occurred. There was little or no anxiety manifested, no dyspnœa, and the voice, frequently hoarse and distinct, was often quite natural. The patient soon became quiet and composed, and allowed the experiment to be indefinitely prolonged.

The symptoms which distinguish the passage of an instrument into the trachea and œsophagus may be thus contrasted:

Trachea.	*Œsophagus.*
1. Suffusion of the face, rapidly increasing to turgescence and lividity.	1. Suffusion of face slight; rapidly subsiding, with cessation of retching and cough.
2. Great anxiety and alarm, not easily pacified.	2. Little anxiety, easily pacified.
3. Eyes wild, staring, and overflowing with tears.	3. Eyes natural, slight suffusion from tears.
4. Cough violent and spasmodic.	4. Little or no cough.
5. Respiration greatly disturbed; inspiration loud, hoarse, and stridulous; expiration attended with violent cough, like that of laryngeal phthisis; ejection of bronchial mucus through the tube; and finally, free breathing through the tube.	5. Respiration little, if at all disturbed; occasionally puffs of air through the tube.
6. *Voice extinguished;* a hoarse whisper, interpreted with difficulty.	6. Voice distinct, often quite natural.
7. Retching slight.	7. Retching and vomiting a common symptom.

So prominent and characteristic are the symptoms which the patient manifests on the successful introduction of the instrument into the air-passages, and so strikingly do they contrast with those witnessed when it enters the œsophagus, that your committee came to regard the rational signs as the surest criterion of the success of the operation. In but a single, well-marked instance did they fail of being exhibited with characteristic intensity. In this case the patient had suffered from a syphilitic ulceration of the air passages, and to this circumstance, together with the enfeebled condition, we may attribute the absence of *severe* symptoms; dyspnœa, aphonia, and lividity of the face were the only evidences of the success of the operation. In Case 30 (Mesmore), the characteristic symptoms were also *less marked.*

We would, therefore, establish it as a rule, to which ordinarily there are no exceptions, that the rational signs, above tabulated, will differentially distinguish the course which the instrument takes, whether in the laryngeal or œsophageal passage.

Other tests than those above enumerated were employed to prove the practicability of this operation, but none of them were found, on trial, free from objection.

The power of the patient to blow out a lighted candle, and to collapse and inflate at will, in respiration, a bladder attached

to its free extremity, through the tube, has been much relied upon as a proof that the tube had entered the trachea. But it was found that when the tube was purposely passed into the œsophagus, the same effects were still produced, though in a much less degree.

The passage of two sponge-armed probangs into the throat, and by the withdrawal of the lower one calculating the position of the other, whether in the œsophagus or trachea, did not, as far as experimented with, yield satisfactory results; for it was not proved, to the satisfaction of the committee, that the sponge probang entered the larynx and trachea.

The sensations of the patient are reliable only when the tube has been repeatedly passed both into the trachea and œsophagus. In these cases, patients have very promptly and correctly decided which passage the instrument was entering. This fact was strikingly illustrated in Case XI., patient, Mary Norton, on whom the operation was repeatedly performed before she was brought before the committee.

On the first and second trials of the introduction of the tube by Dr. Green, she shook her head, indicating that it had not entered the larynx. The operator was satisfied, however, on the last trial, that the tube had penetrated the trachea, and that the sensations of the patient were not to be relied on, when she ejected a portion of the contents of the stomach through the tube, and thus demonstrated not only the position of the instrument, but equally the correctness of her sensation.

It appeared very evidently, also, in the course of these experiments, that the opinion of the operator as to the course which the instrument takes, is unreliable when he trusts to his own senses, and disregards symptoms. We witnessed, in cases 11 and 21, the fallacy of Dr. Green's opinion as to the success of his experiment, though based on so large an experience. In both instances, while positive that he had successfully passed the instrument into the trachea, the patients vomited through the tube, and thus demonstrated his error. But little less demonstrative of the same fact, was the frequent assertion of Dr. Green on first passing the instrument,

that it had entered the trachea, and his subsequent doubts and admissions of failure on a closer examination of the case.

3d. The facility of the operation.

While these experiments proved conclusively to the mind of your committee the practicability of passing tubes into the air-passages, they also afforded opportunities of witnessing the facility with which the operation is performed under different circumstances, and with different instruments.

These we shall consider,

1. As regards the previous preparation of the patient;
2. As regards the instruments employed.

1. Previous preparation of the patient.

It is contended that the facility with which the instrument is introduced, depends much upon the previous preparation of the patient, by frequent applications of the nitrate of silver to the upper part of the larynx. To obviate this objection, the committee met twice at Dr. Green's office to witness the experiment upon patients whom he was treating with local applications to these parts; and at the second visit at Bellevue Hospital, the patients were selected from a class which had been expressly prepared for the experiment by the application of caustic to the throat from three to five times during the preceding week.

The results of these experiments, however, do not sustain the above assertion. The patients on whom Dr. Green had been operating for six months, more or less, were as intolerant of the actual passage of the instrument into the trachea as those first submitted to the trial.

The case of Mary Norton (Case 11) is in point. Although the tube, in her case, had been passed into the trachea half-a-dozen times previously to her coming before the committee, still she exhibited, in the most marked manner, all the evidences characteristic of the introduction of a foreign body into the air-passages when the tube was successfully passed. It should be stated that, in the case of Mesmore (30), who had been long under treatment, the presence of the tube was better tolerated.

2. Instruments employed.

The facility with which catheterism of the air-passages is performed, seems to depend, in a great degree, upon the instrument used; and these we now purpose to consider.

The instruments employed in these experiments were:

1. A tube, consisting of a Hutchings' catheter (No. 10), with a wire stilet, and bent with a curvature corresponding to a circle of six inches diameter.

2. A tube, of the same size, slightly bent at its extremity. This was the instrument selected by Prof. Green, and is the one which he is accustomed to use in practice.

3. A sponge-armed probang, the sponge having a diameter of from one-half to three-quarters of an inch.

The result of the experiments with these several instruments was as follows:

	No. of Trials.	Failed.	Succeeded.	Pr. Ct. of Failures.
Tube with large curve,	13	5	8	about 38
Tube with small curve,	38	35	3	about 92
Sponge probang,	18	18	0	100

From these experiments, it appears that the instrument best adapted to succeed in catheterism of the air-passages is a tube having a large curve, or one shaped like a common catheter; while that least adapted to enter the trachea is the sponge-armed probang. We will briefly review our experiments with these several instruments.

1. *The tube with the large curve.*

This instrument was introduced into the trachea eight times to the entire satisfaction of your committee. The fact that the operators were successful eight times in thirteen trials, proves that *this tube* can be introduced with much certainty into the air passages. In operating, the tongue should be pressed forward by the finger passed over the epiglottis, and the tube passed deliberately and cautiously along the finger into the larynx. The finger was found to be preferable to a spatula in depressing and protruding the tongue.

2. *The tube with the small curve.*

This is the instrument selected by Prof. Green for the

catheterism of the air-passages, and is that which he uses in practice. In these experiments he was the only one who employed it, and the result shows that, even in the most practiced hands, it fails in entering the air-passages in about ninety-two per cent. of the trials.

3. *The sponge probang.*

This instrument afforded the least satisfactory results of any used. Notwithstanding the most persevering efforts with the whale bone slightly bent, as used by Prof. Green, and with patients who quietly submitted to the test of experiment, the results were entirely negative. In no instance did it satisfactorily enter the trachea. In two instances, with the whalebone curved like a common catheter, the sponge was thought to have entered the larynx; but with repeated efforts it could not be forced between and beyond the vocal chords. The suffocation was so great each time as to compel a withdrawal of the instrument.

These experiments, as far as they have been carried, justify the conclusion, in the minds of your committee, that the sponge-armed probang, with the extremity slightly curved, cannot enter the air-passages; but that if the curve correspond to that of a common catheter, it may be made to enter the larynx.

Your committee leave it to the Academy to decide the bearing which these facts have upon the assertions "that this method of medicating the larynx and trachea is accomplished with much ease, and, ordinarily, with great certainty," and that the sponge-armed probang "has been, in a thousand instances, thrust down between and beyond the vocal chords, and has been carried not only through the trachea and its bifurcations, but has been passed, *at will*, into the right or left bronchial divisions."

When it is observed that the tube with which Prof. Green is accustomed to throw injections into the air-passages fails of entering the trachea in ninety-two per cent. of the trials, it cannot fail to appear that medication of these passages, by means of this instrument, is a difficult operation, and seldom crowned with success. Still more striking is the

fallacy of the declaration that the sponge probang is readily passed through and beyond the vocal chords, and frequently, and at will, passed into the right or left bronchial divisions, when, in this series of experiments, it was not once made to enter the trachea.

II. *The Utility of Injections of Nitrate of Silver into the Air-Passages and Tubercular Cavities.*

The practicability of introducing an injection into a tubercular cavity, involves, as a necessary condition, the introduction of a tube into or near that cavity. As your committee have no evidence of the practicability of passing a tube into tubercular cavities in the lungs, they will be excused from considering, on any *merely theoretical grounds*, the question of the probable utility of injections into such cavities.

In three patients, injections of nitrate of silver were thrown into the air passages; two were under Prof. Green's care (Nos. 30, and 7, and 34),* and one was an inmate of Bellevue Hospital.† The first two did not again come under the observation of your committee, and they cannot speak of the results. The correspondence, giving a history of the last case, which terminated fatally, is as follows :—

New York, *April 5th*, 1855.

Wm. Frothingham, M. D.

Dear Sir :—I will thank you to send me an account of the condition of James Griffin, previous to the 27th of January, when the nitrate of silver was injected into his lungs, and his appearance afterwards, up to the time of his death.

Please give me an accurate account of the post mortem appearances, and any thing else of interest touching the experiment in his case.

I remain, your friend,

W. Parker.

Bellevue Hospital, *April 15th*, 1855.

Dear Sir :—In answer to yours of the 5th inst., requesting an account of the case of James Griffin, I send a copy of the notes which I have by me.

* Cases Nos. 7 and 34 refer to the same patient, on two different trials.

† James Griffin.

James Griffin, Æt. 55; stone mason by trade; native of Ireland; admitted January 17th, 1855. Had symptoms of phthisis for about eight months prior to admission. On physical examination, signs of tubercles in both lungs, and of a small cavity at apex of right lung, were discovered. Some suspicion of syphilis, but nothing certain made out. Has chronic inflammation of the fauces, and a small ulcer upon one tonsil. Is somewhat deaf. Has neither night sweats nor diarrhœa. Pulse in the afternoon about 90, but less frequent in the morning. Appetite tolerable, general condition rather weak, but is not confined to bed; takes his meals at the common table, and, living in the second story, goes down to the yard to attend to the calls of nature. Has some dyspnœa after ascending stairs. This was his condition up to the afternoon of January 27th, no material change having occurred since his entrance.

On the afternoon referred to, between one and two drachms of a solution of arg. nit., of the strength of two scruples to the ounce, were injected into his trachea.

I saw him within ten minutes after the operation. He was then endeavoring to find his way down stairs to his ward; his mind seemed much confused, and he could speak with difficulty. Being conducted to the ward, he was shortly after seen by Dr. Stone, yourself, and me. He was then laboring for breath, his lips and hands livid, extremities cold, pulse 120 to 130, and thready. An involuntary evacuation of his bowels, into his pantaloons, had occurred.

By the application of external heat, mustard to the epigastrium, and brandy, and carb. ammoniæ internally, reaction was established in a couple of hours.

The next morning he was too feeble to sit up, and had to be propped in bed, that being the easiest position for him. There was great dyspnœa, with a rapid, weak pulse, and a lividity of surface.

But little could be learned by auscultation; loud bronchial rales disguising other sounds, if others existed. Death took place, Jan. 28th, about 4, P. M., within twenty-six hours after the operation.

Autopsy, seventeen hours after death. Abdominal viscera healthy, except liver, which was a little fatty. Heart and great vessels healthy. Fauces inflamed; larynx and upper part of trachea healthy.

About three inches down the trachea, an intense and minute injection of the vessels of the mucous membrane commenced, and continued down into the minute bronchi.

Nearly all those parts exhibiting the inflammatory blush were obscured by an opaline appearance, similar to that observed on mucous membranes which have been washed with nitrate of silver. This faded away after an exposure of an hour to the air, leaving the intense redness of the membrane still more distinct.

The bronchial tubes, from their third division downward, were filled with a grayish white, slightly tenacious secretion, of the appearance and consistence of coagulated pus. This was not adherent to the walls of the tubes.

On microscopic examination, it was found to contain pus corpuscles in moderate quantity, granular exudation corpuscles, blood globules, epithelium, and an abundance of granular matter.

When the right lung was held in its natural position, the tubes leading to the cavity ascended, and (as well as the ascending tubes of the opposite lung) appeared to have received but little of the injected solution, the inflammatory appearance extending up them but a short distance. The plugs of grayish matter were here co-extensive with the inflammatory blush.

The cavity and adjacent tubes presented no evidence of having received any of the injection.

I remain, Sir,

Yours truly,

Wm. Frothingham.

To Dr. Willard Parker.

CONCLUSIONS.

1. Catheterism of the air-passages dates its history from the time of Hippocrates.

2. The best evidences of the passage of an instrument into the air-passages are the rational signs.

3. The facility of the operation depends upon the kind of instrument used; the tube having a large curve being the best, and the sponge-probang least adapted to enter the trachea.

4. That there is no reliable evidence, in the opinion of your committee, that the *sponge-probang* has been passed through and beyond the vocal chords.

5. That there is no positive evidence that an instrument can be passed at will into the right or left bronchial divisions.

6. That, in the great majority of instances, where injections are supposed to have been thrown into the lungs through a tube, they have passed directly into the stomach.

7. That, as regards the utility of injections of nitrate of silver into the lungs, the facts *thus far developed in the experiments of your committee* lead them to regard the operation as one attended with danger as well as difficulty.

In submitting the above Report, the Committee beg leave to tender their thanks to Prof. Green and to Isaac E. Taylor, M. D., Visiting Physician of Bellevue Hospital, who kindly aided in furnishing subjects and in making the experiments which are appended, and which form the basis of this Report.

WILLARD PARKER, M. D., *Chairman.*
JOHN O. STONE, M. D.
ISAAC WOOD, M. D.
JOHN T. METCALFE, M. D.
} *Committee.*

New York, *June* 14*th*, 1855.

Willard Parker, M. D.

Dear Sir: I still entertain the opinion I expressed at the last sitting of the committee on Dr. H. Green's paper, and which I also repeated to you on the following morning—that the second division of the matter referred to the committee had not received that investigation which would enable them intelligently to report upon it.

You will please receive this as my signature to the *first* division of the Report.

With great respect,

Yours, &c.,

James Anderson.

Letter from Dr. A. H. Stevens, one of the Committee, &c.

The undersigned, one of the committee appointed to report on the subject of medicated injections into the trachea, as performed by Prof. Horace Green, not having had the opportunity to witness all of the facts in relation to this subject that have come under the notice of his colleagues, begs to submit this his separate report. He was present at two meetings only at Prof. Green's office, and witnessed, in addition to some very skillful applications of caustic solutions to sinous passages in the fauces, repeated introductions of the sponge-probang, of the hollow tube, and of nitrate of silver injections through it.

As regards the question of the possibility of the introduction of the probang into the trachea, he did not see enough to enable him to consider the practicability of such introduction as a proved fact. But, of the practicability of passing a catheter into the trachea, he was confirmed in his previous belief, that it admits of no question whatsoever. It was moreover proved that the injection of a solution of nitrate of silver, in quantities equal to two fluid drachms, is quite practicable, with ordinary skill.

In two experiments, performed in his presence by Prof. Green, it was not followed by alarming symptoms, or, so far as he knows, by dangerous consequences—a result quite different from what *a priori* reasoning had prepared him to anticipate. If further experience should prove its entire safety, such experience on a very extended scale and by numerous observers, can alone determine its therapeutic value. But those who may incline to make such experiments will doubtless remember that a solution of two drachms of nitrate of silver of the strength stated to have been employed, ℈ij. to ℥j. of water, would give ℈ss of the solid caustic—enough to make an eschar through the cutis of one inch in diameter. Analogy would lead to the conclusion that so large a quantity, expending its whole force upon the tracheal and such parts of the bronchial membrane as it came in contact with, would be likely to induce inflammation of a grave character. The quantity of caustic actually brought into permanent contact with a stricture of the urethra is a very small fraction, probably not one-twentieth part of a grain. A stick of caustic weighing only four or five grains may be used daily for months with scarcely a perceptible diminution of its bulk, even in hospital practice.

In regard to the question of the practicability of directing the solution to a particular part of the bronchial tubes, or into any cavity connected with them, at the pleasure of the operator, the reporter has seen nothing to lead to an affirmative answer.

All which is respectfully submitted.

ALEX. H. STEVENS,
Of the Committee, &c.

APPENDIX,

CONTAINING THE MINUTES OF THE COMMITTEE APPOINTED BY THE ACADEMY TO EXAMINE INTO THE EMPLOYMENT OF INJECTION OF THE BRONCHIAL TUBES AND TUBERCULAR CAVITIES OF THE LUNGS, BY HORACE GREEN, M. D., ON WHICH THE PRECEDING REPORT IS BASED.

Dr. Horace Green, in a paper on Diseases of the Air-passages, read before the Academy of Medicine in December, 1854, advocated, among other things, the introduction of gum-elastic tubes into the trachea, and injections of a solution of nitrate of silver into the lungs.

The President of the Academy, Dr. Jos. M. Smith, appointed the following committee "to inquire into and examine" the proposed treatment:

Drs. W. Parker, Isaac Wood, Jas. Anderson, A. H. Stevens, John O. Stone, B. F. Barker, and J. T. Metcalfe.

January 8th, 1855. The committee were summoned by the call of their chairman. Present—Drs. Parker, Stevens, Wood, Anderson, Barker, Metcalfe, and Stone.

Dr. Stone was chosen Secretary. It was concluded, after some general discussion of the subject, that the committee should endeavor, for the present—

1st. To settle the question of the passage of the tube into the trachea; and,

2d. As far as possible, to ascertain the utility of the injections of the nitrate of silver into the lungs.

Dr. Barker then informed the committee, that he was requested by Dr. Green to invite the committee to visit his office at any time, after two days' notice, to witness his treatment.

The invitation was accepted, and Dr. Barker requested to inform Dr. Green that the committee would assemble at his office on the following Saturday, the 13th of January, at 11 o'clock. The committee then adjourned.

Saturday, Jan. 13th. The committee met at Dr. Green's, as agreed upon. All were present; but Drs. Parker and Metcalfe did not arrive until after a number of patients had been examined.

1*st Patient*—Mrs. Crumft. In this case it was proposed to show the passage of the probang. The epiglottis was distinctly visible; preternaturally red. The patient was a young woman, in apparently good health, but voice had failed for a number of years. Her voice was found to break in singing the low notes. She had put herself under Dr. Green for hoarseness, and says she is better now than she has been for four years; has pursued the treatment for five or six months.

The instrument was passed with much facility, but could not be distinctly seen by the spectators; very slight cough was occasioned, and only slight strangulation, consequently the operation cannot be regarded as satisfactory.

2*d Patient*—Mrs. Selden—has no children, catamenia regular, no consumption in her family, had great pain before she came to Dr. Green; something seemed to form in the throat, which was followed by expectoration tinged with blood; the cough and expectoration were greatest in the mornings; the sputa yellow, and mixed with blood. Dr. Green had removed the tonsils. The left sub-tonsillary fossa could be seen ulcerated. Dr. Green regards this symptom as an indication that the left (the corresponding) lung is diseased. Nothing *marked* on percussion or auscultation, but perhaps there may be a shade of dullness under the left clavicle, and also a little diminished respiration on this side.

Patient says she is much improved by the treatment; has very slight expectoration, *not* tinged with blood, nor is it so thick as it formerly was; thinks she has gained twenty pounds in four months; has been under treatment six weeks.

On passing the probang, no cough or spasm or suffocation was produced. On withdrawing it, it was arrested, and Dr.

Green directed attention to this fact as an indication of its being embraced by the *vocal cords.* According to the patient's confession, the instrument never excited pain or cough. The course the instrument took could not be seen; consequently, judging from the symptoms, was not satisfactory.

3d Patient—Mr. Moore. Not regarded as tubercular; has been affected with chronic bronchitis for a year last September; never raised blood; has been two or three months under treatment. His flesh has increased within a year.

The tonsils and uvula had been removed by Dr. Green. Epiglottis visible, and rather redder than usual. The tube was passed with a bent wire in it, so as to give it a very slight curvature, and an injection of a solution of nitrate of silver of the strength of thirty-five grains to the ounce, was thrown in.

This tube was withdrawn, and another passed with a bladder fastened to its outer extremity. The air was drawn from the bladder, but it was not inflated again by the patient. In the first operation, the tube may have been successfully passed, but nothing in the symptoms proved its positive presence in the trachea. The second operation with the bladder was unsatisfactory.

4th Patient—Mr. Crocker—subject to epilepsy. Epiglottis just visible. The tube was passed with the bladder attached. Dr. Stone passed his finger into the mouth, and felt the epiglottis and tube, and, to him, it seemed as if the tube had entered the trachea. Dr. Parker, on making a similar examination, was of a different opinion. Dr. Metcalfe, also on examination with the finger, supposed it to have gone down the œsophagus. The symptoms manifested by the patient confirmed their opinion; for he breathed easily, and did not seem to suffer at all. Trial unsatisfactory.

5th Patient—John Brummer, aged 35. Says that at one time he vomited a gallon of blood. The tube was passed without suffocative symptoms; and the nitrate of silver injected. On withdrawing the tube, every drop of the solution was found to be sucked from it, only a slight cough was caused, and a sensation of warmth excited, the patient says, in the left thorax. He does not taste the solution in his

mouth immediately after it is given, nor even after some hours have elapsed.

Dr. Metcalfe designedly passed the tube into the œsophagus, and the patient declared it was in a different place from what it was when placed by Dr. Green.

6th Patient—E. W. Butler, an epileptic discolored with the nitrate of silver, which he had taken some years ago for his fits, after he had left the supervision of the physician who had prescribed it. He applied to Dr. Green for these convulsions, and thought himself benefited by the treatment with the probang. The tube was passed, and he was seized with a slight epileptic fit. The tube was at once withdrawn, and he soon recovered from the attack.

7th Patient—Robert Lathrop. Has never spit blood. On the first of September raised pus. Last winter and this winter, he has gained flesh, having lost flesh during the summer; has gained more this winter than he did last, in amount about four pounds: there was diminished respiration at apex of right lung.

Dr. Green introduced the tube with the intention of entering the right bronchus. The injection of the nitrate of silver was given. The patient says he feels the injection in the right lung; first a cold and then a warm sensation. The matter raised after an injection, the patient says has the taste of the nitrate of silver. The committee did not consider the experiment in this case satisfactory.

8th Patient—Stillwaggen. The probang and tube were both passed: the first caused great irritation; the second very slight. The injection was perfectly sucked from the tube.

The committee then adjourned to meet at Bellevue Hospital on the following Saturday, January 20th, to witness some further experiments under the care of Dr. Isaac E. Taylor. Dr. Green was invited to be present at the same time.

Saturday, January 20th, 1855. The committee met, as agreed upon, at Bellevue Hospital. *Present*—Drs. Parker,

Barker, Anderson, and Stone. Drs. Stevens, Wood, and Metcalfe were absent. Drs. Taylor, Green, Douglass, and Peaslee were also present, besides a large number of other medical gentlemen.

Dr. Green, on being informed that the patients now to be exhibited had never been subjected to any preparatory treatment, and, with few exceptions, had never had caustic applied to their throats by means of the probang, stated that he feared that the experiments would fail. In his own practice, he always considered it essential to success to have the probang with caustic used daily to the throat, for at least a week before any attempt was made to introduce the tube or probang into the trachea.

9th Patient—Bryan Turpin, has a cavity in the left lung. No applications to the throat had been made previously, but an attempt had been made to introduce the tube. The object of the experiment, especially, was to ascertain how much the patient could breathe, and how well articulate, when the tube had been passed through the vocal cords.

Dr. Green was operator. The tube seemed to catch against the vocal cords. There was great spasm and seizing of the instrument by the patient, and great cough afterwards. The patient became alarmed, and desired to be allowed to go. He was persuaded to have another trial made. The instrument caught again, apparently against the vocal cords. In this second experiment there was no spasm, but some cough afterwards.

10th Patient—Larkinson. Epiglottis just visible. No applications had been made to the throat previous to the experiment. Dr. Green introduced the tube, and the patient spoke distinctly, and said that it did not hurt him. There was some cough and disposition to vomit; great cough afterwards. Dr. Green, in order to remove some doubts which were expressed of the tube being in the trachea, proposed to pass one probang into the trachea and another into the œsophagus. Cough was produced. On moving the one (said to be) in the œsophagus up and down, no motion was communicated to the other. Dr. Green regarded this experiment as

proof that both probangs were not in the œsophagus. Dr. Parker stated that he was not satisfied with the experiment, on account of the absence of appropriate symptoms.

11*th Patient*—Mary Norton—has no disease of the lungs, but is a strong, healthy young woman, a servant in the hospital. She had been experimented on before by Drs. Parker and Taylor, who felt satisfied that they had succeeded in passing the *tube* into the trachea. The patient readily volunteered herself for the new experiment. Dr. Green passed the tube. Some effort to vomit was created; she could articulate indistinctly, but could not breathe through the tube. On withdrawing the instrument, she said it went into her stomach. On a second trial by Dr. Green, she shook her head to indicate that it was not in her lungs, and finally ejected (vomited) through the tube. Much cough was produced.

Dr. Taylor bent the tube, so as to form a circle of some five or six inches in diameter, and then introduced it. The symptoms were in striking contrast to those which were produced in the other experiment. There was great cough and spasm, and reddened face; in fact, all the symptoms of impending suffocation. A full current of air was sent through the tube, and a candle was blown out readily. On being encouraged to speak, said "Yes, sir," in a hoarse whisper. Dr. Green uses his tubes but very slightly bent at the point.

12*th Patient*—Stephen Tompkins. Tubercular; no application had been made before. Dr. Green passed the tube; no cough nor much struggling produced. Dr. Green supposed it to have entered the œsophagus. On a second trial with the tube, it was arrested momentarily, but advanced again on pressure. Dr. Green interpreted this arrest to signify its passage through the vocal chords. Dr. Parker then intentionally passed a probang into the œsophagus, and a second one afterwards, but beyond the first. On withdrawing the last, the first was drawn up also. On introducing both probangs, one after the other, into the œsophagus, and passing the second one up and down, the first one held its place. Dr. Barker did not assent to this, and desired to have it recorded

that he was of the contrary opinion. No objections were made by the other members of the committee.

13*th Patient*—James Kerr. Has had nitrate of silver applications to the throat. The object of the experiment was to see, when the probang was introduced into the larynx, how much the patient could breathe. The probang was introduced by Dr. Green. The patient said it had passed into the lungs. Dr. Parker then passed it into the œsophagus, but the patient could not speak. The curved tube was then taken by Dr. Green; but he did not succeed in making it enter the larynx. Dr. Douglass then made an attempt. The tube entered the glottis, and immediately came away of itself.

14*th Patient*—John Wiley, a large and courageous man, with a capacious throat. The epiglottis visible; has had no applications to the throat before. Dr. Green passed the tube. There was no cough or struggle. Dr. Taylor then used the curved tube. *Great spasm and cough, with livid redness of the face*, was caused; the patient could speak his name, but it was in a *croupy and suppressed whisper; he breathes huskily and could blow out a candle readily.*

The tube was then passed into the œsophagus, in order to show that the blowing out of a candle is no proof that the tube is in the trachea. There was no struggling or effort made to vomit. This experiment did not succeed.

15*th Patient*—John Grindle. The epiglottis could be seen and touched readily with the finger. Dr. Stone, having placed the tip of the index finger of the left hand upon the epiglottis, passed the curved tube into the trachea. *Great cough and suffusion of the face* was produced; the patient could *not speak.* The patient withdrew it himself. Dr. Stone then attempted to pass the tube without the wire, but it went into the œsophagus. Dr. Green then passed it into the œsophagus. The patient *could speak but very indistinctly—not in a husky voice.*

Saturday, January 27*th*, 1855. The committee met again, at Bellevue Hospital, for the purpose of making new experi-

ments, but especially to ascertain if the tube could be passed, with more ease and more frequently and with less violent symptoms, in patients who had been subjected previously to frequent topical applications of the nitrate of silver to their throats. It may be remembered that, at the last meeting of the committee, Dr. Green had objected to the patients, because they had not been prepared beforehand, and stated that he considered previous preparation necessary to success in passing the tube. Each patient, to be to-day exhibited, had, since the last meeting, as many as three to five applications made to the throat under Dr. Taylor's superintendence.

The committee present were Drs. Parker, Barker, and Stone. Drs. Green, Douglass, Taylor, T. F. Cock, Peaslee, Gluck, and many other medical gentlemen, not including a large number of students, were also present.

Dr. Green mentioned that, in recent experiments, since last meeting of the committee, he had succeeded best in passing the tube, in new patients, where the tube had a large curve given to it, as used by Dr. Taylor.

16*th Patient*—Michael Gorman. Dr. Green used the tube *slightly* bent, and endeavored to pass it. The patient vomited, but none of the fluid came through the tube. Several trials were made, all of which caused vomiting.

17*th Patient*—John Burns. This patient was excited, and all attempts failed from his want of courage.

18*th Patient*—James Martin. Dr. Green passed the tube. It went into the œsophagus. The patient was timid, and indisposed to have any attempt made.

These patients were nervous and timid. The vomiting of the first excited the rest, and, upon the entrance of the tube into the mouth, they gagged, and became unmanageable. Dr. Green regarded them as unprepared for the experiment; for, as he said, his own private patients never acted in this way, nor did their throats, after previous preparation, manifest such irritability.

19*th Patient*—Stephen Tompkins, experimented upon at the last meeting of the committee. The tube was passed by Dr. Green. The patient could not blow through the tube.

It passed into the œsophagus. On a second trial, it also passed into the œsophagus.

20*th Patient*—John Wiley. Dr. Green passed the tube, more curved than in his usual experiments. The candle was blown out. The patient vomited while the tube was being introduced, and while it was in; but none of the fluid ejected seemed to come through the tube. In this case there was no spasm, or suffusion of the face.

21*st Patient*—Charles Singleman. Epiglottis visible. Dr. Green passed the tube. The patient vomited through the tube. On a second trial, great gagging, and, finally, vomiting through the tube. On the first trial, the tube was supposed by Dr. Green to be in the trachea until the patient vomited, and thus showed that it was in the œsophagus.

22*d Patient*—Anthony Annikin, vomited on depressing the tongue. On a second trial, with the probang, vomited. There was no proof to the spectators that it was in the trachea, although Dr. Green was of this opinion. On a third trial, vomited on depressing the tongue. On a fourth trial, ditto.

23*d Patient*—Patrick Larkin. Dr. Green passed the tube. On first trial it went into the œsophagus. On second trial it was doubtful; the holes in the tube became blocked up with mucus; the patient could not speak. Dr. Green thought it had passed into the larynx.

24*th Patient*—Martin Coyne. On first trial, he pulled out the tube. On second trial, he spoke his name. Apparently the tube was in the œsophagus. On third trial, vomited through the tube, with much struggling.

25*th Patient*—Isaac Griffin, a very deaf man. Dr. Taylor remarked that this man was affected with secondary syphilis and tubercles, and that he had succeeded in passing the curved tube into the trachea; but, on withdrawing, it had assumed a double (sigmoid) curvature. On first trial with the curved tube, Dr. Taylor entered the œsophagus. Dr. Stone then felt for the epiglottis with the left index finger, and passed the curved tube into the trachea. There was no spasm, and the patient breathed freely through the tube. The

success of the experiment was admitted by all. A solution of the nitrate of silver was then thrown in without any spasm. It is proper to mention that this patient was in a very feeble condition when this experiment was tried. This fact may, perhaps, in a measure, account for the little distress caused on the passage of the tube. The syphilitic affection of the trachea undoubtedly contributed, also, to render the parts insensible.

After the committee adjourned, this patient was visited, in the ward, by Drs. Parker and Stone. He was made to hear by applying a flexible stethescope to the ear. He said that he felt worse since the experiment was made. He was much agitated; his sphincters were relaxed, and he had defecated in his clothes. The cutaneous circulation was impeded, and he was enfeebled and exhausted.

26*th Patient*—James McCormick. Epiglottis seen to be thickened; the pharynx red and irritable. Dr. Green tried the tube, slightly bent; but the first trial failed. On second attempt the tube seemed to have entered the glottis, but was pulled out by the patient.

27*th Patient*—John Henry. Resisted, and pulled away the tube. The experiment failed.

28*th Patient*—James Logue, has had the nitrate of silver applied to the throat four times previously. On first trial the patient resisted. Dr. Stone then took the tube, much curved, and, feeling for the epiglottis with the finger, passed the tube into the larynx. The patient breathed through the tube. *The face became suffused, and there was great spasm and threatening suffocation*, so that it was withdrawn almost immediately after its introduction.

29*th Patient*—Mary Norton, the servant-maid who volunteered last Saturday for the experiment, and in whom the tube was successfully passed into the trachea, to every one's satisfaction, came forward, again, for a new trial. Dr. Green passed the tube, bent, as usual with him, a *very little*, and successfully introduced it into the larynx. *There was great spasm, redness of the face, and cough; and, although she has now had the tube passed several times, there was no diminu-*

tion of the symptoms of suffocation. She breathed through the tube with a full current, and spoke her name, when told to do so, in a husky and almost inaudible whisper. Dr. Stone afterwards, designedly, *passed the tube into the œsophagus, and the patient, when told to speak, said, in a loud voice, "it is in my stomach."*

This experiment showed that the tube slightly bent, *can* be passed into the trachea. Dr. Green claims an equal success with all his patients. In all the successful cases, i. e., when the tube passed to the satisfaction of all into the trachea, there was *great spasm, with the exception of the deaf man.*

The committee agreed to meet on the following Thursday, at 1 o'clock, A. M., at Dr. Green's Office, to witness his success in patients that had been prepared for the experiment under his own supervision.

The committee then adjourned.

Thursday, February 1*st*, 1855. The committee met, as agreed upon at the previous meeting, at Dr. Green's Office. Present, Drs. Parker, Barker, Stevens, Anderson, Wood, and Stone. Dr. Metcalfe was absent. Besides these, there were present Drs. Green, Douglass, and Gluck.

At a previous meeting, Dr. Green had objected to the patients as imperfectly prepared for the experiments; that the application of nitrate of silver to their throats had not been made with sufficient frequency, and consequently the patients resisted and rendered it impossible to pass the tube when only *slightly bent* at the point. He promised us a greater success in his own patients, and the chief object of the present meeting was to witness his success.

30*th Patient*—Messmore. Has been at various times, for the last nine months, under treatment; once a week or every two weeks had come to Dr. Green's. An abscess had formed in the right tonsil which had been followed by fistula, through which pus could be squeezed; the tonsil had been removed by Dr. Green at several operations, and latterly the fistula had been laid open with the knife; pus can be made to exude from the tonsil on pressure.

The patient has never had cough; his family have been long lived—none of them tubercular, and his own health is apparently good. His digestion is generally good, but in his wanderings, he has been compelled to live on unsuitable food, and then his digestion has suffered. The patient is interested in his case—talks much about it, and has made it an especial study—has even dissected the dead body for information, and thinks he can tell by the difference of the sensations produced, whether the tube passes into the trachea or œsophagus, and also can tell, by his sensations, that the injections of the nitrate of silver pass into the ramifications of the bronchia.

The epiglottis is visible on depressing the tongue, and the tube can be seen entering the glottis. The tube was passed without much struggling, and air was freely expelled through the tube. An injection of two or three drachms of the nitrate of silver of the strength of thirty to forty grains to the ounce, was then thrown in. All present were satisfied, I believe, that the experiment was successful. The patient, a few minutes afterwards, coughed up the injection; or, rather, what he raised up had the taste of nitrate of silver.

31*st Patient*—Mr. Crocker, an epileptic. On the first trial by Dr. Green, the patient spoke distinctly, consequently the tube was believed to be in the œsophagus. On the third trial, Dr. Stone, with a finger on the epiglottis, passed the curved tube into the trachea. The patient *could not vomit*—there was great *suffusion of the face* and *spasm*, which, after a little while, became allayed—*air was driven in a full current through the tube.* It was conceded that the tube was fairly in the trachea. The injection with the nitrate of silver was not given. On withdrawing the tube, there was much irritative cough, and Dr. Green said that he would allay it, by passing a probang with the sponge dipped in nitrate of silver into the glottis. This was done, and success followed the experiment. This patient had presented himself to the committee at the previous meeting at Dr. Green's. Dr. Green repeated the statement that the curved tube could be passed in new and unprepared patients; but

that the tube, slightly bent, could be passed with equal facility in those who had been properly prepared.

32*nd Patient*—Mr. Stillman. Has had the injections of the nitrate of silver fifteen or twenty times—usually feels no sensation afterwards, except when the stomach is empty, when there is slight nausea. He thinks he can tell whether the tube is in the trachea or œsophagus. Has had severe bronchial troubles, but no tubercles have been discovered. He did have some night sweats until he came under Dr. Green's care; but never to any great extent; has raised some discolored sputa tinged with blood, but no blood since he has been under Dr. Green's care. Raises about two ounces of sputa daily, which is an increase compared with what there was formerly; has gained five and a half pounds of flesh.

The tube, *more than usually curved*, was passed by Dr. Green. Great spasm was caused, and the tube withdrawn by the patient. After a lapse of a considerable period of time, during which the committee was engaged in examining other patients, Mr. Stillman was tried again. *He objected to the curved tube as causing unusual suffocation.*

Dr. Stone passed the tube without the wire, and the patient expelled air through the tube, but very slightly and not in a full current. It was believed to be in the œsophagus. The patient was excited, as were some of the others by the presence of so many spectators, and the failure of the experiment was attributed by Dr. Green to this unusual circumstance.

33*rd Patient*—Dr. Pulling. The tube was passed by Dr. Green. The patient could not say positively whether it was in the œsophagus or trachea. It was probably in the œsophagus. After the lapse of a little time, Dr. Pulling consented to allow Dr. Stone to try the curved tube while his finger was upon the epiglottis. The epiglottis could be reached with difficulty, and only occasionally. The experiment was a failure: the instruments passed into the œsophagus.

34*th Patient*—Robert Lathrop. Experimented upon at a previous meeting of the committee, when, although the injection was given, the committee did not consider the experi-

ment satisfactory. To-day, Dr. Green fairly passed the tube into the trachea. The injection was thrown in: *throughout, the spasm and cough were great.*

The patient said the effect was *different from that usuallg produced*, and the *cough greater*, and the sensation, after the injection, higher up in the chest on the right side. To explain this last symptom, it was mentioned that the tube had not been so far introduced as usual.

The committee consulted together, and after having received an invitation from Dr. Green to call singly at his office and examine his practice, adjourned.

March 23d., 1855. The committee were called together by their chairman, and met at Dr. Stone's. Present, Drs. Parker, Wood, Metcalfe, and Stone. Dr. Anderson was absent, as were Drs. Stevens and Barker, who had gone out of town. The notes of the experiments made at different sessions of the committee were read, and much discussion followed. The chairman was anxious to gather the opinions of his associates, in order to be aided in making his final report.

It was conceded that the tube *could* be passed into the trachea; but in order to decide when this was successfully done, there was *nothing to guide us but the symptoms manifested. These were cough, livid redness of the face, strangulation, husky voice, croupy whisper, and breathing through the tube, in a full current.*

June 5th, 1855. The committee met at Bellevue Hospital. Present, Drs. Parker, Anderson, Barker, and Stone. Dr. Wood came in during the experiments on the first patient; Dr. Metcalfe entered while the second patient was being experimented upon. Dr. Stevens was absent. Dr. Green, who had been invited to meet the committee, was also absent.

Drs. Taylor, Stephen Smith, Gourley, and four other gentlemen belonging to the service of the hospital were present.

35th Patient—Elizabeth Peasler, aged 25. Face pale; has diminished respiration, with prolonged expiration, at summit of the right lung. Cough ever since last summer.

Phthisis in its early stage—no cavity. No instruments had ever been used before. Epiglottis visible on depressing the tongue with the spatula. The probang was passed by Dr. Taylor. She breathed easy; no suffusion of face or redness; some sickness, but no vomiting. The instrument was kept in after its introduction, for five minutes. Respiration a little embarrassed at first, but soon became calm and regular; could speak indistinctly but not croupily. The curved tube one quarter of an inch in diameter was used, in order to see if any different symptoms were manifested. Dr. Tay- employed the instrument moderately bent, with a wire in it. The instrument, guided by the index finger on the epiglottis, caught in the glottis, causing some suffusion of the face and lachrymation, but it slipped immediately afterwards into the œsophagus. While in the glottis, the symptoms of suffocation were intense; face red and livid, and struggling—great cough afterwards; and an avowal on the part of the patient that the instrument was in a very different place from what it was in the first experiment. It was apparent from the symptoms that in the first trial it went into the œsophagus, and in the second into the glottis.

The probang used was one obtained from Dr. Green.

36th Patient—Melvina Mosier, aged 24. Slight cough for two months. No disease of the lungs discoverable. The patient is affected with primary syphilis. Epiglottis visible on depressing the tongue. Dr. Taylor attempted to pass Dr. Green's probang (this instrument is nearly straight, being only slightly bent at the point); a little cough was caused and some efforts to vomit, but no suffocation. It passed into the œsophagus. A curved probang (not belonging to Dr. Green) was next used, to show that *it* could be made to enter the trachea, and that accompanying symptoms would indicate where it had gone. First attempt did not succeed. Second attempt, Dr. Stone tried with his finger on the epiglottis, it seemed to catch in the glottis, causing suffocation, and then to slip into the œsophagus. Third trial with curved *tube.* It just entered and then slipped out. Fourth trial with curved *tube* was unsuccessful. Fifth trial with probang bent over a

lamp so as to be largely curved. No suffocation caused; she breathed easily, it evidently had passed into the œsophagus. Sixth trial. A curved tube again. It passed into the œsophagus. This was inferred from the absence of suffocative symptoms. In this patient no previous applications had been made to the throat.

37*th Patient*—Catherine Phillips, aged 24. The tube was introduced on Saturday last, June 2d., into her trachea by Dr. Taylor. She has no cough or diseased lungs, but is suffering from primary syphilis. Epiglottis is visible on depressing the tongue. Dr. Stone made two attempts with Dr. Green's probang. Both times it passed into the œsophagus. The epiglottis *could not be touched with the finger.* Vomiting was caused by the withdrawal of the instrument. Dr. Taylor tried the *curved* probang—it passed into the œsophagus. Dr. Barker tried with Dr. Green's probang while the tongue was depressed with the spatula. His object was to pass the instrument rapidly, and thus, as it were, surprise the glottis, before any spasm could arise. He failed. Dr. Barker tried again with the slightly curved *tube.* She bore it well for a time; ejected air from the tube, and from this circumstance, although there was no suffusion of the face or suffocation, some thought it was in the trachea—she finally vomited, which showed this supposition to be incorrect. Dr. Barker admitted his belief that it had passed into the œsophagus. Dr. Taylor then tried the curved tube, and from the redness of the face, rapid movements of the body and limbs, and inability to breathe, there was no doubt that the instrument was in the trachea.

38*th Patient*—Mary Hewitt, aged 22 years. Has no cough. The curved probang was used by Dr. Taylor; it passed into the œsophagus. Tried again, and from the livid colour of the face and partial asphyxia, there was no doubt it was in the larynx. Tried again with the tube largely curved; it entered the glottis but could not be passed further. From spasm, &c., inferred that it had entered the glottis. In this patient one application had been made previously to the throat.

39*th Patient*—Catherine Phillips. The patient before the

last, was brought back, and Dr. Parker with finger on the epiglottis, tried the curved probang. The instrument rested for a moment on the glottis, and then slipped into the œsophagus and caused vomiting.

The minutes taken at this session were then read, and after some conversation, the committee adjourned to meet at Dr. Parker's at 8 o'clock this evening.

The committee met at 8 P. M. according to adjournment. *Present*—Drs. Parker, Stone, Wood, Anderson, Barker and Metcalfe. Dr. Stevens was absent. The report was then read and assented to. The committee then adjourned *sine die.*

JOHN O. STONE, *Secretary.*

MINORITY REPORT.

At a meeting of the New York Academy of Medicine, Dec. 6th, 1854, the following resolution, furnished to me by the Recording Secretary, was passed, viz.: "Resolved that a committee of seven be appointed by the chair to enquire into and investigate the treatment proposed by Dr. Green, in his paper read this evening."

In the paper referred to the committee, Dr. Green first alludes to the scepticism of Professor Erichsen, of London, and Professor Trousseau, of Paris, as to the possibility of "passing the sponge-probang through the larynx into the trachea, even of the cadaver, much less into that of the living subject."

Dr. Green claims "That this method of medicating the larynx and trachea, is accomplished with much ease, and, ordinarily, with great certainty; and that, by this means, diseases of those organs, which would otherwise have proved mortal, have been repeatedly arrested." He goes still further, in asserting that the sponge-probang "has been passed at will into the right or left bronchial divisions." To prove that it could be passed through the vocal chords, Dr. Green attached a sponge, of the same size with those which are used with the ordinary throat-probang, to one of Hutching's flexible tubes, and wetting the sponge with a strong solution of nitrate of silver, he passed it down to the vocal chords, through the rima-glottidis (as he supposed) several inches into the trachea. The patient breathed through the tube for several moments, and extinguished a lighted lamp, by blowing through the

tube. In order more rigidly to test this experiment, in another patient, a large pasteboard card, perforated in the center, and of sufficient size to screen the nose and mouth completely, was then slipped over the tube, to which it fitted closely, and the patient directed to blow out the light, which was accomplished through the tube as promptly as in the first instance. In another patient, a small air-tight elastic bag was tied over the upper extremity of the tube, and on introducing the instrument six or eight inches into the trachea, this little bag was inflated and collapsed several times, by the act of inspiration and expiration on the part of the patient. In another case, a small ball of floss was suspended by a thread before the mouth of a tube, and this was alternately drawn into and expelled from the opening by the act of respiration.

Dr. Green believed that these experiments conclusively proved "that the operation of passing the sponge probang into the larynx and trachea is positively being accomplished." From these results he was led to enquire, "What shall now hinder the introduction of medicinal agents through the lungs, or directly into the bronchi and their terminations? What will prevent the injecting even of a vomica, *under favorable circumstances*, with appropriate remedies?"

Dr. Green at once endeavored to put in practice the inferential suggestions resulting from the experiments above alluded to. In the paper referred to the committee, he professes to have "treated for a longer or shorter period thirty-two patients laboring under tubercular or bronchial diseases, by the direct introduction into the lungs of a strong solution of nitrate of silver, injected through the elastic tube."

He details with considerable minuteness three cases, "whose history and sanatory condition, on coming under my (his) treatment were known to other medical men." He claims that this treatment has been "invaluably salutary," both in the cases of chronic bronchitis, and in those of tubercular diseases; or, to be more specific, he asserts that this treatment produces a marked decrease in the cough and expectoration, an abatement of most symptoms which arise from local inflammation or irritation in the pulmonary apparatus,

and *consequently*, a decided improvement in the general health of the patient. Many important points in regard to the treatment he leaves "to be solved by future experience, and by repeated observations." Dr. Green adopts the following proprositions of Professor Bennett, of Edinburg, from his work on the "Pathology and Treatment of Tuberculosis":—

1st. That, not unfrequently, diseases, entirely seated in the larynx or pharynx are mistaken for pulmonary tuberculosis.

2nd. That even when pulmonary tuberculosis exists, many of the urgent symptoms are not so much owing to disease in the lung, as to the pharyngeal and laryngeal complications.

3rd. That a local treatment may not only remove or alleviate these complications, but that in conjunction with general remedies, it tends in a marked manner, to induce arrestment of the pulmonary disease.

The conclusion of Dr. Green's paper contains the following three distinct propositions:

1*st. That direct medication of the lungs, by means of catheterism of the air tubes, has not before been accomplished.*

2*d. That the operation may be performed by the dexterous surgeon with ease and facility, and with perfect safety to the patient.*

3*d. That the results of this method of treating disease, whether it has been employed in bronchial affections or in the commencement of tuberculosis, have already afforded the most gratifying indications that practical medicine will be greatly advanced by this discovery.*

This condensed summary of the paper referred to the committee, seems necessary in order clearly to define the duties of the committee. They are, manifestly:

1st. To ascertain whether direct medication of the lungs, by means of catheterism of the air tubes, has been, before, accomplished.

2d. To ascertain whether the operation may be performed by the dexterous surgeon with ease and facility, and with perfect safety to the patient.

3d. To ascertain, as far as possible, the therapeutic results of this method of medication.

That the committee clearly understood their duties to be as above stated, is proved by the record of the secretary, Dr. Stone, from which the following paragraph is an extract:

"It was concluded, after some general discussion of the subject, that the committee should endeavor, first, to settle the question of the passage of the tube into the trachea; and, second, as far as possible, to ascertain the utility of the injections of the nitrate of silver into the lungs."

The title of the paper referred to the committee is "on injections of the bronchial tubes and tubercular cavities of the lungs." The practicability of passing the sponge-probang through the vocal chords was not even suggested by any member of the committee, as pertaining to their duties; but, as a majority of the committee have declared, by appending their names to the report, "that there is no reliable evidence, in the opinion of your committee, that the sponge-probang has been passed through and beyond the vocal chords," it is proper, before the Academy shall endorse such a statement, that they enquire whether the committee have investigated this subject, and what they mean by "reliable evidence."

Neither Dr. Green, nor any other advocate of the practicability of passing the sponge-probang through the vocal chords, professes to do it without first overcoming the irritability and sensibility of the parts by "repeated applications of the nitrate of silver to the faucial and pharyngeal region, to the epiglottis, and about the opening of the glottis." To investigate this point, then, it is necessary that the above condition should be fulfilled. The record of the secretary proves that this was not done in the patients experimented upon at the first and third meetings of the committee, at Bellevue Hospital. At the second meeting of the committee at Bellevue Hospital, those experimented upon had received, some three, and some four, applications of the nitrate of silver; but that the sensibility and irritability of the parts had not been overcome, was disgustingly evident to all present. The record says: "these patients were nervous and timid; the vomiting of the first one excited the rest, and, upon the

entrance of the tube into the mouth, they gagged, and became unmanageable."

The experiments, then, at Bellevue Hospital, have no possible bearing on the question, and should be excluded. At Dr. Green's office, the sponge-probang was avowedly passed in two patients before the committee. Their investigation, then, is confined to two cases, and from these they deduce a proposition which they ask this Academy to endorse.

Let us next see what they mean by "reliable evidence." In regard to the first patient in whom the probang was passed, the record says "very slight cough was occasioned, and only slight strangulation, consequently the operation cannot be regarded as satisfactory." In the second case, the record says: "consequently, judging from the symptoms, was not satisfactory." The symptoms were negative; "no cough, spasm, or suffocation was produced." The committee say, "the best evidence of the passage of an instrument into the air-passages are the rational signs." They give, in full, what they deem the "rational signs;" but these, surely, cannot apply to the sponge-probang with an impervious handle. They will not claim that a patient, with such an instrument passed through the vocal chords into the trachea, will have a "loud, hoarse, and stridulous inspiration;" that the expiration will be "attended with violent cough, like that of laryngeal phthisis;" that there will be "ejection of bronchial mucus through the *tube*, and, finally, free breathing *through the tube*." Striking out the words "through the tube," the phenomena which the committee have described as "rational signs" of the passage of an instrument into the air-tubes, almost exactly characterize a paroxysm of hooping-cough. Still more perfectly do they describe the effects of a first application of a strong solution of nitrate of silver to the faucial and pharyngeal surface, in most patients, when the probang does not pass at all into the air-passage. It necessarily follows that the "rational signs" of the committee are, in no sense, "reliable evidence" of the passage of a sponge-probang, through the vocal chords, into the trachea. Contenting myself, therefore, with the positive assertion that I have re-

liable evidence that the sponge-probang has, in many instances, "been passed through and beyond the vocal chords," I deem it no part of my duty, as a member of *this* committee, to bring that evidence before the Academy.

The committee were appointed "to enquire into and investigate the treatment proposed by Dr. Green in his paper," read before the Academy, Dec. 6th, 1854. That treatment is the injection of nitrate of silver into the bronchi, and "under favorable circumstances," into tubercular cavities. I now proceed to what I consider to be the legitimate duty of the committee.

1st. To ascertain whether direct medication of the lungs, by means of catheterism of the air tubes, has been before accomplished.

On this point little need be said, as I presume the Academy and the committee will unanimously concur in the statement that, in the history of medicine, there is no evidence that this has ever before been accomplished.

2nd. To ascertain whether "the operation may be performed by the dexterous surgeon with ease and facility, and with perfect safety to the patient." The operation consists of two parts—1st, the introduction of an elastic tube through the vocal chords into the trachea. 2nd, the injection, by means of a small syringe, of a solution of nitrate of silver, through the tube into the bronchi. The latter part of the operation can, of course, be accomplished with ease and facility. The first part, then, only requires examination, and this is not new to medical science. One of the "conclusions" of the report of the majority of the committee is, that "catheterism of the air passages dates its history from the time of Hippocrates." If this be true, it would hardly seem necessary that in the year of grace, 1855, a committee should enter upon a series of experiments to ascertain its "practicability."

But I am ignorant of the data upon which the committee base such a conclusion. Hippocrates (*de Morbis, lib. III.* 10) gives the following advice: "Fistulæ in fauces ad maxillas intrudendæ, quo spiritus in pulmonem trahatur." "Canulas should be carried into the throat along the jaws, so that the

air can be drawn into the lungs." But M. Sestier (*Traité de l'Angine Laryngée Œdemateuse,*") has shown that it is very doubtful whether the ancients reallly did pass the sound into the trachea. He remarks that αυλίσκος, diminutive of αυλος, sound, means a small sound, and that under the denomination of φαρυγξ, the ancients meant sometimes the *throat*, sometimes the *pharynx*, and sometimes even the *larynx* Hippocrates then advises the introduction of small sounds, but whether into the *throat*, *pharynx* or *larynx*, it is impossible for us to decide. At all events, the practice was altogether abandoned when Asclepiades proposed bronchotomy. It was almost lost sight of when Desault, in 1801, revived it, since which time it has been really introduced and left for a longer or shorter time *in the larynx and trachea*, and we have recorded in the annals of science many cases where the tube has been passed into the trachea as a remedial measure in the cases of impending suffocation. Prof. Lallemand resorted to this method in two cases, one of which is reported by Dr. Sestier, and the other by Dr. Justin Benoit. The latter also operated in this way in one case of his own. Desault reports three cases, Giraud two, Bichat one. Another case is reported by Desault as occurring in the Hospital of Lyons, in which a soldier, who had attempted to kill himself, was *nourished* and *breathed* fifteen days by means of two sounds, one introduced into the *œsophagus* and the other into the *larynx*. A sufficient number of cases have been enumerated to establish the fact that this part of the operation can be performed by the dexterous surgeon with ease and facility. Mr. Ryland, who is alluded to in the report of the majority of the committee, thus speaks of the introduction of a tube into the trachea. "The idea of the aperture of the glottis being closed by the abasement of the epiglottis upon it, has long since been exploded, and it is now well known that a tube, passed in the manner just indicated, will, if directed by a scientific hand, readily find its way into the trachea."

With such ease and facility does the tube pass into the trachea, that it often occurs as an accident, where the intention of the operator has been to pass it into the œsophagus,

and this too, with a tube much larger than those Dr. Green makes use of. Dr. Watson, at the last meeting of the Academy, mentioned one case of his own, where this happened in one fourth of the operations, if I correctly understood him.

The experience of Dr. Watson is confirmed by many other surgeons. There are cases recorded where the tube has been introduced into the trachea and produced so little disturbance, that the operators have believed it to be in the œsophagus, and injected liquids into the air-passages, thus producing most unfortunate accidents, and even fatal results. It is sufficient for me to refer to a Thesis by M. Prus, Paris, 1817, No. III, p. 26., to the articles by M. M. Thoré and Baillarger, in the *Annales Medico-psychologiques*, T. V. et VIII. So well is this known to surgeons, that Prof. Miller, of Edinburgh, in his standard work on the "Practice of Surgery," gives especial directions to prevent the occurrence of such a mistake. The following are his words:—"In most cases, it is well to assure ourselves fully that the tube is in the œsophagus and not in the larynx, before fluids are passed downwards into the stomach. For this purpose a sheet of paper may be placed before the face, with the extremity of the tube projecting through it, while in front of the tube a lighted taper is put, which by the paper is effectually secured from the flatus of the nostrils in expiration. If on expiration, the flame remains steady, no air impinging on it, we may proceed with the injections, the tube is certainly in the right place. If the flame be extinguished or even made to bend considerably, it is equally plain that an error has been made, and that injection would almost certainly occasion fatal asphyxia." The facts above mentioned conclusively *prove that the absence of the "rational signs" as given by the committee*, is *no evidence that the tube is not in the trachea, and that when the committee have decided on these grounds that an experiment was unsuccessful, their decision is wholly unreliable.* I shall recur to this point again in examining the record of the committee. Enough has been said to prove the practicability of the operation. I submit however, in addition the following letter which I this morning received from Prof. Peaslee.

HANOVER, June 16th, '55.

DEAR DOCTOR,—As I perceive by the city papers that the subject of passing tubes and sponge-probangs into the human trachea is just now exciting a good deal of interest, I send a brief statement of a case in which the patient has *himself* introduced the tube into his own trachea and injected the nitrate of silver through it. The gentleman alluded to, is a *physician*, and has long been affected by follicular disease of the pharynx, with subsequent atrophy of the tonsils, and extension of the former disease into the larynx and trachea. He was formerly my pupil, and I place full confidence in his opinion, that he has passed both the sponge-probang and the tube into his own trachea. He also is positive that the former has been passed many times by others before he accomplished it himself. The tube was first passed with the assistance of a medical pupil, and afterward, by himself unaided, the pupil merely assisting in the subsequent injection.

On inquiring how he could be *certain* the sponge-probang or the tube, passed through the *larynx*, he replied that he had had the former as well as the latter too often slip down the œsophagus, instead of entering the trachea, not to know the difference by the sensations at once. No extreme symptoms in the way of cough or dyspnœa were produced by the introduction of the tube. The gentleman to whom I alluded is Dr. D. B. French, of Bath, N. H.

Yours truly, in haste,

E. R. PEASLEE.

Dr. B. F. Barker.

Let us now examine the experiments of the committee. As the great difficulty in passing the tube arises from the sensibility and irritability of the parts involved, Dr. Green never attempts the operation until this difficulty has been overcome by previous education of the parts. It is obvious that in investigating *his* plan of treatment, this prepared condition is an essential requisite for all *fair* and *reliable* experiments, just as much so, as it is for the chemist, who proposes to analyze any given substance, to have his retort free

from all extraneous matter. But this condition was not fulfilled in any of the experiments at Bellevue Hospital, and therefore they might with justice be excluded, as having no bearing upon the subject. But as the committee have made use of them, we will see what these experiments prove. Twenty-two patients were subjected to the experiments with the tube.

For this occasion only, I accept the decision of the committee, and set down as "failed, successful, and doubtful" as they determined. For convenience in referring to Dr. Stone's record, I have marked the patients as numbered by him.

Failed.	Doubtful.	Successful.
13th Patient	9th Patient	11th Patient
16th "	10th "	14th "
17th "	12th "	15th "
18th "	23rd "	20th "
19th "		25th "
21st "		26th "
22nd "		28th "
24th "		29th "
27th "		34th "

The experiments failed in nine patients, were successful in nine, and the results were doubtful in four. I respectfully ask of the Academy to examine the causes of failure as they appear in the record. Fifty per cent. of success where the results were decided. The success was more wonderful than the failure, success being achieved under circumstances where Dr. Green would not attempt the operation in his own practice, the failure being due to causes which are not inherent to the operation, but which might have been overcome. The success proved the practicability of the operation. The failure proved nothing but the great sensibility and irritability of the parts, which condition was greatly increased from *emotional* causes. It would seem hardly necessary to say that for the operation to be successful, it is requisite that the patient should be free from mental perturbation. The experi-

ments at Dr. Green's office were undoubtedly less satisfactory on this acount than they otherwise would have been. The presence of an unusual number of physicians, the discussion as to the possibility of the operation, and the test of its success, all served to excite the patients and embarrass the operation. The only cases where failure was positive are marked 32 and 33 in the record, and, in these, the failure was wholly due to emotional causes. A letter from one will subsequently be introduced. The sixth should be excluded, as immediately after the introduction of the tube, the patient, an epileptic, the well known "Blue Man," had a slight attack of epilepsy, and the tube was at once withdrawn. In the remaining ten cases, the decision of the committee was that the results of the experiments were doubtful in four, viz: in the 3rd, 5th, 7th and 8th. In regard to the 8th, no opinion was expressed by the committee. I therefore set this down among the doubtful. The experiments failed in three, viz: the 4th, 32nd and 33rd. In three, the 30th, 31st and 34th, the experiments were successful. Here, as before, where the results were decided, the success is exactly 50 per cent. But were their decisions correct as regards those considered as doubtful, and in the 4th patient decided as a failure? Their decision was based on the absence of certain phenomena which they denominate "rational signs." But these have already been shown to be *wholly unreliable.* Their absence proves nothing. The *probabilities*, mathematically speaking, are as eight to two that these were all successful, and these probabilities are greatly increased by the phenomena observed, as noted by Dr. Stone, to which record I refer for proof. We are therefore fully warranted in claiming success in the eight out of ten patients in whom the experiments were tried at Dr. Green's office. It remains to enquire whether, in the two unsuccessful results, the failure was inherent to the operation. In one (the 32nd), the patient, a highly intelligent man, had had the tube passed repeatedly, both into the trachea and into the œsophagus. According to the committee, he was competent to decide by his sensations, into which passage the tube is passed, and he avers that it has been many times pas-

sed into the trachea. In regard to the other failure, I will introduce the following correspondence:—

New York, *June* 19*th*, 1855.

Dear Doctor,—As your name appears in the Record of Dr. Stone, Secretary of the committee "on Injections into the Bronchial tubes, &c.," as one in whom the experiment of passing the tube into the trachea was made before the committee, may I beg of you to inform me by letter: 1st, whether you believe the tube has been passed through the vocal chords into the trachea? 2d. Also, whether the injection has been made through the tube into the bronchi; and, if so, what, in your opinion, were the effects of this method of treatment? 3d. Whether at the time referred to by Dr. Stone, in your opinion, the tube was passed through the vocal chords into the trachea?

I remain, yours truly,

B. Fordyce Barker.

Dr. E. R. Pulling,
Resident Physician to the New York Lying-In Asylum.

New York, *June* 19*th*, 1855.

Dear Sir,—I have before me your note of the present date, in which you make some inquiries in reference to the introduction of the tube into the air-passages in my own case.

While I wish to shun the unfortunate position of a *partizan* in the discussion of the question now before the committee, I yet see no valid objection to stating such particulars as have a bearing on the subject of your note.

Dr. Green attempted to pass the tube into the trachea, on three occasions. The first attempt was unsuccessful. The second was successful; the tube, I think, undoubtedly passing through the vocal chords and into the trachea. An injection was thrown in on this occasion, for the purpose of relieving a bronchial irritation from which I was suffering. The effects following this operation, were, during the first six or eight hours, an increased expectoration, the cough remaining about

the same. At the end of this period, the cough and expectoration began to diminish, and for several days subsequently, I found much relief from previous symptoms. The third attempt at passing the tube, made in the presence of the committee, was unsuccessful. A result which, so far as I am capable of estimating its causes, was due to spasm of the vocal organs, induced by fatigue, and increased by the circumstances under which the operation was performed.

Very respectfully, yours,

E. R. Pulling.

To Dr. B. F. Barker,
Member of the Committee of the Academy of Medicine.

Enough has been said to establish the fact "that the operation may be performed by the dexterous surgeon with ease and facility."

As to the *safety* of the operation, it is sufficient to say, that it has been performed by Dr. Green, in more or less patients daily, for more than six months, and this too, constantly in the presence of physicians and patients from all parts of the country. With the strong partizan feeling enlisted in this subject, if a single unpleasant result had occurred in his practice, it could not have been concealed. It would have been trumpeted throughout the length and breadth of the land. But as nothing of the kind has happened, the safety of the operation may be regarded as fully established.

3rd. The *therapeutic results* of this method of medication.

No one fact is better settled in the science of therapeutics, than the effect of nitrate of silver on inflamed mucous membranes. The pathological condition of the mucous membrane of the air tubes, in chronic bronchitis and in the bronchi leading to tubercular deposition, is equally well established. Most physicians accustomed to treat pulmonary diseases, must have had, in the course of their practice, some such ideas as are happily expressed in the following quotation from Dr. Chambers, a distinguished physician of London:—

"While treating disease in those parts of the mucous membranes which are sufficiently exposed to sight and touch, for

the immediate application of remedial agents, there are few to whom the wish has not occurred that equal facilities were afforded of directly influencing the deeper seated continuations of the same fabric. The powerful remedies which restore so rapidly to health the conjunctiva and the fauces, would probably act with equal quickness and success on the stomach or the bronchi, could we apply them rightly to the right spot, and attack the local disease, without passing circuitously through the whole system."* *A priori* then we should look for most favorable results from this method of medication as meeting "a general indication, which the judicious practitioner will know how to avail himself of."†

The writer of this report has abundant proof that the results of experience fully confirm the expectations founded upon analogy and inductive reasoning. He has availed himself of every opportunity to watch the effects of the treatment now under discussion. He has examined a large number of patients, has learned their commemorative symptoms, and has watched their progress from day to day. A report of this character cannot be burthened with a minute record of individual cases. General results only can be stated. The use of the injections has uniformly been followed by a marked decrease in the cough and expectoration. A large proportion of the cases were those of chronic bronchitis. There were some of early and some of advanced phthisis. In all the cases of bronchitis, the patients began to improve as soon as the injections were used. Many of them recovered with a rapidity which I have never seen under any other treatment. In early tuberculosis, especially when complicated with bronchial disease (as it usually is), this method of topical medication has produced the most gratifying results, diminishing the cough and expectoration, relieving dyspnœa, and allaying pain in the chest when present. In several prominent cases, the details of which, at some future time, will be given to the

* Lond. Lanc. (Am. Ed.) 1849. Vol. 10, p. 29.

† The Path. and Treat. of Tuberculosis. By John Hughes Bennett, M. D. &c., p. 140.

profession, the disease appears to be arrested. Time, of course, must determine this point. In some advanced and hopeless cases of phthisis, the injections have for a time greatly diminished the cough, expectoration, and irritation about the chest, and some of these patients and their friends have repeatedly declared that they invariably rested better after an injection. Many physicians of this city, and from different parts of the country, will fully confirm the above statements from their own observation. Some, among whom may be mentioned, Drs. Bowditch and Perry, of Boston, and Marsdon, of Quebec, have, to a limited extent, tested this plan of treatment in their own practice, with equally happy results, as far as their experience goes.

In conclusion, the minority of the committee would suggest for the acceptance of the Academy, the following propositions:—

1st. Direct medication of the lungs by means of catheterism of the air-tubes, was first proposed and carried into effect by our associate member, Dr. Horace Green.

2d. The operation may be performed by the dexterous surgeon, with ease and facility, and with perfect safety to the patient.

3d. The results of this method of treatment, whether it has been employed in bronchial affections or in the commencement of tuberculosis, have already afforded the most gratifying indications that practical medicine will be greatly advanced by this discovery.

B. Fordyce Barker.

ON THE TREATMENT

OF

PUERPERAL CONVULSIONS.

By B. Fordyce Barker, M. D.

read dec. 5, 1855.

In the following paper, an attempt is made to formularize the treatment of puerperal convulsions, based on the pathology, as accepted in the present state of science. While the semeiology of this fearful complication of parturition has been familiar to the profession from the earliest ages of medicine, its pathology has only very recently been understood. Even M'Clintock and Hardy, who are among the latest of our standard authors on "Midwifery and the Diseases of the Puerperal State," say, "The pathology of puerperal convulsions, as well as of the other diseases in the nosological class to which it belongs, is a subject upon which we are in almost complete ignorance."

Hence its treatment has been purely empirical. The recorded statistics show that few diseases have been treated with so little success; 32 per cent. proving fatal where the attack has occurred before and during labor, and 22 per

cent. where the convulsions have come on after labor.* There is no complication of labor which has been attended with such fearful mortality.

But the experimental researches of Majendie, Flourens, Marshal Hall, and other investigators, among whom our own Dalton should be mentioned as one of the most zealous and successful laborers, have entirely revolutionized the physiology of the nervous system. The pathology of the nervous system is consequently a new science; and to Dr. Tyler Smith, in my estimation, are we more indebted than to any other author, for our advance in the right direction in developing the application of the new discoveries, to explain the phenomenon of the disease of the nervous system now under consideration.

We shall, therefore, first, briefly review the pathology of puerperal convulsions. All convulsions arise from some irri-

*	Total.	Before & during Labor.	Recov'd.	Died.	After Labor.	Recov'd.	Died.
Mauriceau,	45	29	13	16	16	11	5
Mad. La Chapelle,	27	23	16	7	4	2	2
Desjardin,	7	5	5		2	2	
Velpeau,	21	12	8	4	9	5	
Smellie,	10	10	7	3			4
T. Clarke,	19	17	12	5	2	2	
Lever,	14	12	8	4	2	2	
Robert Lee,	54	46	31	15	8	7	1
Ramsbotham,	25	22	14	8	3	3	
Collins,	30	28	23	5	2	2	
M'Clintock & Hardy,	13	10	7	3	3	3	
TOTAL.	265	214	144	70	51	39	12

* Authors have differed as to the comparative mortality of convulsions occurring before and during labor, and those coming on after delivery. Mauriceau, Velpeau, Dugès, Nægele, Churchill, Murphy, &c., regard those cases which come on after delivery as much more amenable to treatment than those which occur before and during labor; while Smellie, Astruc, Tissot, Ramsbotham, and some others, hold a contrary opinion. The table I have given above shows that 32 per cent. of those cases occurring before and during labor proved fatal, while only 22 per cent. died where the convulsions came on after delivery.—*From an article by the writer, on the "Use of Chloroform in Puerperal Convulsions," N. Y. Medical Times, Vol. 2, No. 9.*

tation of the true spinal system, which includes the spinal marrow within the theca vertebralis, the medulla oblongata, and the corpora quadrugemina. No irritation of the cerebral system—that is, of the brain and cerebellum and that part of the spinal chord which conveys sensation and voluntary motor power to and from the brain—will produce convulsions. Puerperal convulsions differ in certain essential characteristics from all other forms of convulsions, the difference resulting from some peculiar condition of the nervous system, developed during the periods of gestation, parturition, and lactation.

Puerperal convulsions have been regarded and described by many authors as being of an epileptic character. But, as Prof. Murphy has clearly pointed out, while epileptic and puerperal convulsions nearly agree in the form of attack, they differ in the manner of their incursion and in the ultimate course they take. To quote from Dr. Murphy, "Epilepsy agrees with puerperal convulsions in—

1. Violent convulsions of the voluntary and respiratory muscles;
2. Total loss of consciousness;
3. Lividity of features from apnœa;
4. Followed by temporary coma.

They differ from each other in the following characters:

IN EPILEPSY,

1. An aura precedes the attack;
2. There is no hissing expiration;
3. Fits return periodically, at long intervals;
4. The paroxysms are seldom fatal;
5. Epileptics usually give evidence of some pre-existing constitutional derangement.

IN PUERPERAL CONVULSIONS,

1. Symptoms of cerebral congestion precede the attack;
2. Hissing expiration very characteristic;
3. Fits return in rapid succession;
4. The paroxysms are often fatal;
5. The healthiest women are often attacked."

Other writers again, among the most prominent of whom is Dr. Ramsbotham, regard puerperal convulsions as being allied to apoplexy. But neither local congestion, nor the pressure on the brain resulting from serous or sanguineous effusion, will produce convulsions unless the pressure be directly on the medulla oblongata. Stupor, stertor, coma, or paralysis, may result from apoplexy, but not convulsions. Apoplexy not unfrequently *follows* puerperal convulsions, but does not cause them. As Dr. Marshall Hall has clearly shown, the convulsive fit has the effect of interrupting the circulation,—1st, by the direct pressure of the platysma myoides on the blood returning from the brain; 2d, by the spasm of the glottis impeding respiration, and preventing the passage of venous blood into the lungs; 3d, by the pressure on the venous circulation of the extremities, the blood by the spasmodic contractions of all the voluntary muscles being forced too rapidly forward into the great central trunks; 4th, by the increased pressure on the venous circulation in the uterus, in consequence of its more powerful contractions.

Puerperal convulsions also are caused by an exactly opposite condition from apoplexy, viz. anæmia. The final symptom in death from uterine hemorrhage, is ordinarily convulsion. In animals killed by blood-letting, convulsions occur during the act of dying. So where there is a deficiency of nutrient blood in the system, the exhaustion of the vital powers from labor may have the same effect in producing convulsions as extreme hemorrhage.

The causes of puerperal convulsions are divided by Dr. Tyler Smith into the *centric*—those that act directly upon the true spinal centers, and the *eccentric*—or those that act indirectly, through the agency of some distant organ, upon the spinal system.

The centric causes, are,—

1. Pressure upon the medulla oblongata from congestion, from coagula, or from serous effusion within the cranium;
2. Anæmia, or deficient nutrition of the spinal system;
3. Toxæmia, or an impure condition of the blood.

Dr. Tyler Smith also includes emotional causes; but irritation of the brain from shock can only act indirectly, or in a reflex manner.

The investigations of late years seem to prove that toxæmia is the most frequent of the direct causes. In a large proportion of cases, the albumen is drained from the blood in the urine, while the urea is left. Thus in nine cases, reported by Dr. Geo. T. Elliot, albuminuria existed in seven, and the experience of other observers is very nearly the same. The presence of urea in the blood in these cases, has been conclusively demonstrated.

In a recent case, occurring in the practice of Dr. Sayre, the blood, after standing, emitted a strong uric odor; and on evaporation, it was found loaded with the various crystals of urea.

The *eccentric* causes of puerperal convulsions are morbid excitation of the peripheral nerves of any of the vital organs, and irritation of the brain from shock. Their order of frequency is as follows, basing the order on a careful analysis of all the recorded cases of puerperal convulsions accessible to me.

1st. Irritation of the incident spinal nerves of the uterus and uterine passages;—as from distention from the liquor amnii; pressure of the fœtal head on the cervix uteri and vagina.

2d. Irritation of the incident spinal nerves of the rectum;—as from accumulation of fæces, &c.

3d. Irritation of the brain from shock, joy, terror, &c.

4th. Irritation of the gastric and intestinal branches of the pneumogastric nerve,—as from indigestible food, &c.

5th. Irritation of the incident spinal nerves of the bladder,—as from retention of urine.

In puerperal convulsions we have ordinarily a combination of one of the centric with one or more of the eccentric causes. Thus, irritation of the nerves of the uterus or of the uterine passages, the most common of the eccentric causes, will rarely produce convulsions unless there is either hyperæmia, anæmia, or toxæmia.

The above condensed exposition of the pathology of this disease, was deemed necessary in order to philosophically discuss the treatment.

TREATMENT.—*Prophylactic.*

M'Clintock and Hardy have well remarked, it is a most happy circumstance that, in a disease so justly dreaded and so full of danger as puerperal convulsions, there very generally exists some precursory symptoms of a sufficiently obvious character to lead one to anticipate its attack, and by the timely use of proper remedies, to prevent it altogether, or materially lessen its violence. Warnings of this kind are very seldom absent, although they are not always equally striking or manifest. The most constant of these premonitory symptoms are headache, varying in kind and degree, but generally of a dull, obtuse, or tensive character, and liable to be increased on exertion, particularly on stooping; an œdematous condition of the face and upper extremities, most visible soon after rising in the morning; a furred tongue, and sluggish state of the bowels. At the present day, every intelligent physician, on finding œdema of the face and hands would test the urine for albumen. If, in addition to the symptoms above enumerated, there were vertigo, tinnitus aurium, flashes of light before the eyes, muscæ volitantes, temporary loss of vision or of consciousness, flushed face, pain at the epigastrium, and an albuminous state of the urine, active prophylactic treatment should be at once commenced. If the patient is plethoric, or there is excited vascular action, venesection should be resorted to. The bowels should be freely evacuated, and kept in a soluble state. Indeed, all the depuratory functions ought, during gestation, to be increased; as the debris of the fœtal, as well as the maternal system, have to be eliminated by the organs of the mother.

The following combination I have found of great value in these cases, after venesection, and, indeed, in some instances, as a substitute for blood-letting: ℞ James' powder, grs. iv; Soda bicarb., grs. iij; P. Digitalis, gr. j; M.—to be given

three times a day. In addition, the patient should be placed on a restricted diet, the bowels should be kept well opened, and she should be encouraged to take as much out-of-door exercise as possible.

Unfortunately, the medical attendant frequently does not see the patient until labor comes on. The signs which should then awaken the attention of the vigilant physician are, great restlessness and impatience, especially at each recurrence of pain, so that it is with great difficulty the patient can be restrained from flinging and tossing herself about; the manner is often changed, and unlike what is natural to her. "At other times, there will be temporary loss of consciousness, described by the nurse as a faint. Rigor and headache are frequent concomitants at this time, and the pulse is generally found to be uncommonly slow or considerably quickened." The physician should now carefully seek to ascertain the centric and eccentric causes of this condition, and to remove these by well-selected prophylactic measures. If there is evident hyperæmia, as shown by the strong, full, bounding pulse, venous turgescence of the face and neck, the hot skin, the flushed face, and the injected conjunctivæ, venesection should be promptly resorted to. But a careful discrimination should be exercised between the pulse of irritation, evidence only of nervous excitability, but generally accompanied with a hot skin, and flushed and turgescent face. If any of the eccentric causes are found to exist, as improper food in the stomach, constipation, or a distended bladder, they must be promptly removed. If there is indigestible food in the stomach, it should be removed by an emetic of sulphate of zinc. But an emetic should never be given in a threatened attack of puerperal convulsion, without absolute proof of its necessity, and rarely until after venesection; as the very act of vomiting might produce cerebral congestion. If the intestines are loaded, they should be at once freely evacuated. But the method of accomplishing this is a matter of the greatest importance. The irritation of the intestinal canal by drastic cathartics, may be a most powerful reflex excitant of convulsions. There is little difference between irritant drugs and irritant

fœcal matter. A copious enema of warm soap and water, to which one or two ounces of castor oil may be added, acts almost immediately, without irritating the bowels. The state of the bladder should be carefully examined, and, if necessary, the catheter should be used. But the great source of reflex irritation causing the convulsions, is the uterus. The discriminating physician will readily decide when the liquor amnii should be evacuated by rupturing the membranes. This accomplishes for the uterus what an enema effects for the rectum. The distention of the organ is removed, diminishing its size and the quantity of blood circulating in it. But the great prophylactic measure, after all, is the use of *chloroform.* It has been supposed by many, that a tendency to cerebral congestion contraindicates the use of chloroform. But, on the contrary, sound reasoning and clinical experience conclusively show, that a tendency to cerebral congestion in parturition is a decided indication for the use of chloroform. By its use, the spasmodic contractions of all the voluntary muscles, which contribute so essentially to force the blood to the head, is overcome. The contraction of the platysma myoides, the pressure of which prevents the return of the blood from the head, is also overcome; and, lastly, the tendency to spasm of the glottis, which impedes respiration and prevents the passage of venous blood into the lungs, is prevented. After inhalation of chloroform, I have repeatedly seen the swollen, flushed face become calm and tranquil, the bounding, rapid pulse become soft and natural, the hot skin become cool, and the patient, who was before restless and irritable, tossing about from one side of the bed to the other during the recurrence of each pain, now lying in apparent sweet repose, while the uterine contractions were still going on with the utmost regularity. Were it not that this paper would thus be made unnecessarily tedious, the detail of several such cases might be given. But I doubt not, the experience of many members present will furnish numerous verifications of the above statement. Indeed, I may be permitted to state, that I have never known an attack of puerperal convulsions *during labor* where the precursory

phenomena were sufficiently evident to lead to the adoption of appropriate prophylactic treatment, and the patient has been brought under the influence of chloroform. In the patient of Dr. Sayre, the premonitory symptoms were very striking; but the danger was warded off by the use of chloroform during labor. Some hours after the labor terminated, and the use of the chloroform had been suspended, she had a very violent convulsion, which left her in a state of coma. She was bled very largely; but the stertorous breathing continued, with a constant tendency to convulsive movements. She was kept then under the influence of chloroform. Gradually her breathing became quiet, and the convulsive movements ceased. Opium was then principally relied upon for the subsequent treatment, and she made a perfect recovery. I have already mentioned that the blood in this case was loaded with urea.

Treatment of the attack.—The indications are, 1st. To remove the cause of the spinal irritation, whether it be centric or eccentric, or a combination of both. 2d. To allay the morbid irritability resulting in convulsions already developed. We shall consider the treatment under each of these heads. The centric causes, as has already been stated, are hyperæmia, toxæmia, and anæmia. We shall now consider the remedies which have been empirically sanctioned by the profession, and endeavor to ascertain their true value and appropriateness. 1st. Blood-letting—This is perhaps more universally adopted in the treatment of puerperal convulsions than any other remedy, and in a certain class of cases, it is the most important and effective, both to *cure* the spinal and to *prevent* cerebral disease. But there is no doubt that it is often most injurious in its effects, the loss of blood reproducing the convulsive seizures, acting as a centric cause. Let us attempt to determine the laws which should regulate the use of this measure. Where there is a great fullness of the vascular system, venesection is a powerful sedative of spinal action. Where the disease results from stimulation of the spinal system by excess of blood, or from the mechanical pressure of blood on that organ, or from counter-pressure of the distended brain upon the medulla

oblongata, bloodletting alone is often sufficient to subdue the disease. It, in these cases, is also equally important to preserve the brain from injury from the convulsion. The attack may, as in a manner before shown, cause such turgidity of the vessels of the head, as to result in fatal cerebral congestion, or serous or sanguineous effusion. But where there is an anæmic condition of the system, either pre-existing, or induced by hemorrhage during labor, blood-letting is a *stimulant* of spinal action, and would not only aggravate the convulsions, but greatly increase the danger to the brain from serous effusion. So also in those cases where it was clearly indicated in the first instance, its repetition may change its action from a sedative to a stimulant of spinal excitability. In hyperæmic convulsions, after one bleeding, sufficient to fully impress the system, vascular excitement may be kept down by the use of the tartrate of antimony, as proposed by Dr. Collins: Two grains of tart. antimonii dissolved in four ounces of water, to which is added one scruple of tinct. opii, to prevent diarrhea from following its use. A table spoonful of this mixture is given every half hour or hour, according to the urgency of the symptoms.

Where anæmia is the centric cause, exhausted nervous power, to use a somewhat paradoxical phrase, is the stimulant to spinal action. Here, opium in a full dose is the grand remedy. It restores nervous energy, and thus allays spinal irritability. There has been a great discrepancy of opinion among authors as to the propriety of using opium in the treatment of puerperal convulsions, some condemning its use in the strongest terms, while others highly extol it for this purpose. This difference of opinion has arisen from an imperfect understanding of the pathology of the disease, and a consequent lack of discrimination in the application of the remedy. Not only is opium a most valuable remedy in anæmic convulsions, but it is also frequently of great service in hyperæmic convulsions, after blood-letting. Nervous power has been exhausted, not only by the convulsive attack, but by the necessary blood-letting; and opium, in restoring nervous power, allays the spinal excitability.

It is unnecessary again to refer to the proper treatment for

the removal of the eccentric causes, as this has already been discussed in speaking of the prophylactic treatment, with one exception. The exception referred to, is where uterine irritation is the eccentric cause. This is no doubt the most frequent of all these causes. The propriety of *artificial delivery* often becomes a question of the gravest import. The principle should be, whenever artificial delivery can be effected with less irritation than would be produced by the continuance of the child in the parturient canal, it should be effected. Following this law, the decision must be based on the peculiar features of each individual case.

To fulfill the 2d indication, viz. to allay the spinal irritability already developed, we have no therapeutic agent at all comparable in efficiency with chloroform. For this purpose authors have recommended opium, cold affusion, counter-irritation, the various antispasmodics,—as camphor, ammonia, the ethers, musk, assafœtida, turpentine, &c.; but they are all feeble and inefficient as compared with the anæsthetics. Prof. Simpson has ingeniously suggested, that it may aid in removing one of the centric causes, viz. toxæmia.

He says, "If the blood-poison, which in albuminuria produces convulsions and coma, be, as Frerichs believes, carbonate of ammonia resulting from decomposition of urea, can we account for the power of chloroform in restraining and arresting, as it does, puerperal convulsions, upon the ground of its preventing this decomposition? The inhalation of chloroform produces, as various chemists have shown, a temporary diabetes, sugar appears in the urine, and hence probably also in the blood. The addition of a little sugar to urine *out* of the body, prevents for a time, the decomposition of its urea into carbonate of ammonia."

Whether future researches prove this hypothesis to be true or false, facts have been sufficiently accumulated to establish beyond controversy that the use of chloroform does restrain and arrest puerperal convulsions in a large proportion of cases. Some have been disappointed on finding that it did not have this effect in all instances. Where the convulsion is the

result of direct pressure on the medulla oblongata, or where the convulsion produces serous or sanguineous effusion, the chloroform can have no influence in controlling the fits. Where, then, there is *complete* coma, and especially when there is partial paralysis, no good effect can be anticipated from the use of chloroform.

No judicious man would think of using the chloroform in anæmic convulsions; as in allaying the spinal excitability, there would be great danger of overwhelming the nerves of organic life, and thus destroying the life of the patient. In hysterical convulsion, the chloroform is the sole therapeutic agent required.

It can hardly be necessary to allude to the importance of removing all emotional causes. The room should be darkened, and kept perfectly quiet; no conversation should be permitted; all signs of excitement should be absolutely banished from the room; and the physician should throughout the whole preserve a calm, undisturbed demeanor.

PRIZE ESSAY

ON

CHOLERA INFANTUM.

By JAMES STEWART, M. D.,
Author of "A Practical Treatise on the Diseases of Children."

Si quid novisti rectius istis,
Candidus imperti; si non, his utere mecum.
Hor., Epist. vi. 67.

Cholera Infantum is usually regarded as a disease peculiar to the United States; and its appearance in the cities throughout our land is expected with as much certainty as is the season to which it owes its existence. With this certainty as to the time of its appearance, is to be added the uniformity it observes in its prevalence at a distinct period of life; both showing the unchangeable nature and uniform operation of the agents which are active in its production. No summer passes without leaving evidences of the power a season can exert over human life, and how remarkably that power is active in the development of a peculiar affection at a distinct period of existence.

Most endemic diseases attack indiscriminately at all ages; this one, as its name indicates, is confined to the tenderest age, while there is no disease that simulates it in its most marked characteristics at any other age. The kindred affection that has of late scourged so many countries, is uncertain as to the time of its invasion, and erratic in its course in different countries, yet apparently observes some law of pro-

gression; while the cholera of infants never yet has failed to commit its ravages when the proper atmospheric condition for its development exists, and is as steadily fixed to its locality as any of the natural products of a climate. Such is the experience of American physicians, and it is by them alone that any description of the disease has been given.

It has been stated that a disease exists in England among infants, similar to that which has so much occupied the attention of practitioners in our country. If this be so, it must be vastly less in extent and virulence than the disease as it prevails in the United States; for we find nothing in the way of description, but in its place, surmises that an infantile affection of the bowels existing there, is the same as that called cholera infantum. Had there been the same certainty of the annual return of the disease or any approximation to its mortality, there would not have been wanting numerous essays on so interesting a subject from the profession in Great Britain. It is also said that Dr. Cleghorn described among the diseases of Minorca, one similar to that which prevails so extensively here during the summer among infants; if so, it would appear to prove that the like disease did not exist in Great Britain, or it would certainly have been noticed by the distinguished Dublin Professor, who was at one time an associate in London of those world-renowned observers, Fothergill, Cuming, and Russell.*

French writers say that there is nothing analogous to the disease in France; and when considering it in systematic works on children's diseases, give a description of it derived from American authors.†

In the absence, therefore, of testimony to the contrary, there seems to be some foundation for the opinion that it is peculiar to the United States.

* See Appendix, A.

† "Cholera is not generally noticed in our climate; it is a disease peculiar to infants in the United States; and as I have not observed any thing analogous to this affection, I must be indebted to Dr. Dewees for the principal details of the symptoms."—*Billard on Infants. American edition,* p. 328.

In the course of our remarks on the nature of the disease, it will be seen that at a particular period of life an exaltation of certain functions will arise in the course of the natural development of the body, which often results in derangements of the alimentary canal. This action becoming aggravated, will bear a very close resemblance to the disease in question, especially in its first stage, yet still wanting some of its characteristic symptoms. These symptoms appear to be the result of the combined action of the different external causes of the disease, which constitute the physical conditions of a country in its relations to organic life. The presence of these agents is necessary to produce the disease in all its power: when they are absent, an imperfect development of it occurs, as is doubtless the case in the instances referred to above.

Etiology.

That a clear idea of the causes of this disease may be obtained, with a view to investigate its pathology and the strictly practical application of remedial measures, it is proposed to subject all the circumstances which are observed to control its development to a careful examination. There is no disease which appears to demand so close an investigation of this nature, for there is none which has its pathology so intimately connected with the action of the agencies which are in constant exercise in its production, and in its maintenance throughout its course. The combination of these active agents,—constituting what is known as climate, the more special local agents, and the physiological peculiarities of the period of life at which the disease occurs, it will be seen are so connected in the production and maintenance of the same pathological results, so formidable in their action, and so obstinate in their management. These circumstances are not to be regarded as mere isolated facts, but as an important group, which are essential, when connected with the phenomena of the disease and its morbid anatomy, to illustrate the morbid condition of the system: none of them can be separated

without impairing the relation they bear to the nature of the disease.

Observation 1st.—*Atmospheric Heat.*—The most common attendant on the development of the disease, is a high temperature.

In the city of New York, during a period of eleven years out of twelve hundred and forty-five deaths from cholera infantum, ten hundred and sixty-one occurred during the months of July, August, and September, the three hottest months,—and but six during the three coldest.

The next highest in number as influenced by a high range of temperature, but which includes all ages, is dysentery.

The depression of temperature exhibits a difference also in its effects, but nothing in comparison with that caused by a high range. Thus croup presents the greatest difference from this cause, and of twelve hundred and thirteen deaths presents in no month less than sixty-six,—in the three summer months, two hundred and twenty, and in the three winter months no greater increase than three hundred and ninety-eight.*

In the entire number reported, none shows so great a difference from temperature as the disease under consideration.

So uniformly has this disease existed in the city of New York during the hot months alone, that there is no year in which there is any record of diseases, that deaths from it are not reported as steadily increasing in number with the increase of temperature. For fifty years past, from 1804 to 1854, a large number of deaths has been officially reported from cholera infantum during the summer season, with the exception of one year, when there appears in the official register but one death from this cause. This occurred in the year 1816. On looking over the periodicals and newspapers of that year, it appears that the summer of 1816 was singularly cold, the thermometer up to the 25th of July being from 15° to 20° below the summer temperature, and after that period the mean temperature was but 61°—that, of

* See Appendix, B.

hree months 68°; a sudden elevation for a day or two raising he mean, while the actual continuation of the cold for the eason was without a parallel.* Cholera infantum appears carcely to exist when the mean temperature is about 60°.

Although the mean is usually given as an evidence of the ffect of temperature in producing disease, yet it is obvious iat it is more correctly attributable to the diurnal range. he mean temperature of a week, can give but little informa- on as to the effects of heat when the thermometer has been t or above 90°. The high temperature of 95° will produce erious and often suddenly fatal disease, while the mean of 5° could furnish no explanation.

The invasion of cholera infantum occurs immediately ter a period of excessive heat, and is one of its most marked fects.

Observation 2d.—*Excessive humidity of the air.*—The ndition of the atmosphere with respect to its humidity is ie of the elements which imparts to a country its pe- liarity as regards disease. There does not appear to be iuch difference in the annual mean in various parts of the rthern section of our country, and the diseases incident to i present much the same characters. The humidity, however, i much modified, and often immoderately developed under te occasional changes which occur in the atmosphere; but ore especially is it increased in circumscribed localities and i crowded dwellings.

The temperature at which moisture is deposited, is usually tken to ascertain the amount of moisture in the air. When te temperature of deposit, or as it is termed dew-point, is igh, and especially when it continues high for some time in ht weather, the system feels its depressing influence. All psons suffer from a sensation of lassitude and sweltering oppression at such times; but no class equal to young chil- den. During the sultry nights of summer, in the over-

* See Appendix, C.

crowded habitations of the poor, they show the effects in their excessive pallor, languor, and exhaustion.

In our investigations on the subject of atmospheric humidity, and its relation to the disease under consideration, we have examined various localities, and found as was expected, that a great difference existed. As to the city of New York, we have carefully compared the observations made for the Smithsonian Institution by Prof. O. W. Morris, of the Deaf and Dumb Asylum, with those which we have taken; and have come to the conclusion that ordinary climatic humidity has but limited influence on the development of cholera infantum. The same amount of moisture often exists in country places as is found in the city, and where the disease was never known to originate. We therefore directed our attention to the occasional state of the dew-point as it occurred where the disease was most prevalent, and discovered a great difference within-doors, between it and the general dew-point of the external air, continuing often for a long time.

The first observation made was that the moisture was always greater nearer the surface of the earth; the difference we have ascertained to be at times 4°. Cellars and basements must therefore always be in a state of excessive humidity, as is known to most persons, requiring no philosophical instrument for its detection.

The second is that in very hot weather in excessively crowed houses at night, when all are within, the dew-point is very nearly at the temperature of the air, consequently the air is saturated with moisture. With a temperature of 90° to 95° and a dew-point in a crowded room almost equal to the temperature, a feeling of suffocation is experienced, which is easily accounted for when it is known that the dew-point of the breath as it is expelled from the lungs is 94°, and that the mean dew-point of the atmosphere is 38°; and also, that in the hottest weather it rarely exceeds 70°. When the air is loaded with moisture and deposited easily at a temperature approaching to that of the living body, inspiration is difficult and unsatisfactory, while the system suffers great depression

The effects will be again referred to, when the pathology of the disease is considered.

It is in such heated and moist places that we have found the greatest number of instances of cholera infantum.

Observation 3d.—*Malaria.*—This cause has been regarded as the principal agent in the production of cholera infantum; meaning by it an atmosphere infected by marsh effluvium. It is ascribed to this by some writers, and also by the Committee "On the Epidemics of Tennessee and Kentucky," in their report to the American Medical Association in 1853. This report, and that made in 1854, are the most recent published remarks upon the subject. It would seem, however, more in accordance with well-known facts to consider the *malaria of crowded places*, as the kind of malaria which produces the disease. The report first referred to appears to admit this to a certain extent, as it speaks of the air of apartments that have been carefully closed at night to keep the external malaria excluded, as being the places where the medical practitioner is frequently called to see the disease.

Cholera infantum in some mitigated form may arise in the country, since it will appear in the course of this essay that many of its causes will exist in different places under the influence of a high range of temperature, and different grades will probably be found in places remote from each other. There is, however, so great a modification and so peculiar a type out of the cities as to make it more of a severe diarrhœa, complicated with fever, receiving its characteristic type from the condition of the air producing it, but differing much from the virulent disease as it occurs in cities.

Inasmuch as the usual cause of endemic fevers in the country has been regarded as the cause of this disease, a reference to the nature and source of the two kinds of malaria, with the mode in which their effects are manifested, will be necessary to exhibit them in their proper character, and by comparing them, endeavor to ascertain whether there exists any dissimilarity. This can be very briefly done by avoid-

ing the consideration of every thing that can complicate the inquiry, especially of all subjects connected with the discussion of the pathology of miasmatic diseases, and by keeping closely to the inquiry into their origin and their effects.

The malaria of the country is produced by emanations from marshy places, and by a dessication of the surface of the ground both by solar heat and by the passage of currents of air over it, carrying into the body of the atmosphere the minute gaseous results of vegetable decomposition. That which is exclusively of the city, it is obvious, cannot be produced in the same manner, the surface of the streets not admitting of such constant evaporation by the solar rays or by the passage of currents of air; indeed the absence of the sun's rays, and that also of the movements of the air, are always considered as the principal causes of the diseases that are peculiar to cities. If air from malarious districts is borne into the cities, it must be very considerably modified by the mixture with that which is always found in cities and other crowded places.

That of the country is derived principally from a vegetable source, together with the saturation of the soil from time to time with rain, and its evaporation and diffusion. The animal element forms the principal part of that of the city; arising from the changes which the air undergoes from the depuration from the bodies of living animals where many are crowded together, and the decomposition of dead animal matter. Slaughter-houses, soap manufactories, the refuse of stables, sinks, cess-pools and other receptacles of a similar kind, are among the common sources of the bad air of cities, and which impart to the mass of the air qualities not elsewhere to be found.

The sensible effects of the two are different. In general there is no odor in that of the country, while in the malaria of cities it is decidedly manifest.

It has been remarked (History and Cure of Fevers, by R. Jackson, part i., chap. iii.) that the miasma of crowded places differs from that of a paludal nature, by being less volatile, and confined to a more limited space; and that it is more con-

centrated in its effects, producing an exhausting and depressing action in a more decided manner.

The morbid effects are different.

In all parts of the world, the tendency to diseases in the country is to an intermittent or remittent type, such as fevers, the common head-ache, and neuralgic affections. Those of the city assume more frequently a continued form, even where there is no specific contagion manifest.

The action of malaria from the soil is remarkable also for the production of epizootic diseases. From the time of the Latin poets and historians to the most recent medical reports and essays of the present day, in every country, we hear of extensive diseases that have prevailed at different times among quadrupeds, from paludal effluvia. Such diseases and such mortality are very rare in cities. We have heard of sickness among horses in the vicinity of a city during a pestilence; but a decided epizootic is of rare occurrence and of very limited extent within its bounds; and where an epidemic prevails in such places, the simultaneous sickness among quadrupeds is recorded as a remarkable event.*

In the country, malaria would seem to require a temporary or transient cold state of the air, or of the body, to produce its effects on the system. So universally remarked is this, that the exposure to the cool night air is considered as sure to produce a malarious fever; and it has been referred to the cooling of the body by terrestrial radiation, when soldiers have been taken sick who slept on the ground without tents or other covering, while those who have had bushes, trees, or tents over them have escaped. It cannot be the same malaria that produces cholera infantum as witnessed in cities, inasmuch as it always attacks the greatest number while the heat is greatest, and no danger is apprehended from its temporary mitigation.†

In the country, disease from malaria mostly attacks adults;

* See Appendix, D. † See Appendix, E.

infants born in such places are in general exempt. The reverse is the case in cities; children are the most subject to its malarious influences, and the natives are the first to experience their effects when the season arrives to set them in action. The great mortality from cholera infantum shows that there is but little exemption from its effects among them.

If this disease is caused by the same influences that produce malarious fevers, it would prevail at the time such fevers exist. On the contrary, the greatest number of cases occur in July and August, and when they begin to lessen in number, the paludal fevers increase. We should also expect to find it prevail more during the season when cases of autumnal fever occur in the greatest number. This, however, is not the case, where we have had opportunities of obtaining the most reliable information. Whatever amount of sickness exists in the country surrounding the city of New York, cases of cholera infantum observe a remarkable uniformity as to numbers, evidently under influences different from those producing the febrile affection. In the year 1828, malarious fever prevailed very extensively and with great severity in the country around that city, proving fatal in a great number of instances. Many of the villages were nearly destitute of inhabitants, farms were offered for sale or to lease by their terror-stricken owners, and persons unaffected by the disease were scarcely in sufficient numbers to attend upon the sick. Hundreds fled from all parts, even from the immediate vicinity of the city, and took refuge within its precincts. Various parts of New Jersey, Staten Island, Westchester county, and the regions particularly near the city, were the principal places affected by the disease. It also prevailed with great severity on Long Island, where it was attributed to malaria from decaying vegetable substances exposed to great heat. Several communications appeared in the daily newspapers on the subject, and they speak of noxious vapors of which a single inspiration was injurious.*

* See Appendix, F.

During that year, the mortality from cholera infantum in the city of New York was one hundred and sixty-one, or one in thirty-one of the whole number of deaths; not so great as in the two previous years, when they amounted respectively to two hundred and twenty-two and two hundred and thirty-eight, or one in twenty-two of the deaths. This was also the case in other years; a reference to which is made in the Appendix.*

The disease we are considering appears to differ essentially in miasmatic places from that which occurs in cities. The number of cases of cholera infantum "under 15 years," of which the statistics are given in the report above referred to, as occurring in 1852, in the States of Kentucky and Tennessee, is *fifty-two.* The average attendance of fourteen of them was *four days*, and of seven, *nine days.* The period of attendance required by the remainder is not given, while the mortality of the whole number was *four.* It would seem therefore to differ from the disease known by the same name in cities.†

In the report on the diseases of the same district for the year 1853, one hundred and thirty-five is the number given for the entire State of Kentucky. The necessity there stated for using quinine in its treatment indicates also a very essential modification of the disease in the western part of the United States. The report also states that on a comparison with other diseases that prevail there, cholera infantum has a nearer relationship to dysentery and typhoid fever than to bilious fever; from which there would seem to be an admission that there is some other exciting cause than that which produces paludal fever.

Cholera infantum appears in diminished numbers in proportion to the distance from crowded places, and is most successfully treated when the patient is removed out of the limits of the city even to a short distance; but if the disease came from one and the same cause, and that cause marsh miasm, no mitigation would occur by moving into the air

* See Appendix, G. † See Appendix, H.

which, from its greater proximity to the source of the poison, would be more active in producing its deleterious effects.

From the facts now stated, so far as cholera infantum is dependent on external causes, it is evident that in addition to high range of temperature and inordinate atmospheric humidity combined, it has for its cause the peculiar malaria of crowded places.

Observation 4th.—*Period of Life.*—Cholera infantum prevails more before the first year than during any other period. Although there are no published statistics that show the mortality at the early months, yet it is well known to all practical men that the disease prevails less during the first six months and more from that time to half of the first year. From the first to the end of the second year it exists with varied intensity in different individuals, and after the second year the number is small.

In the city of Philadelphia, during a period of twenty years, prior to 1827, there were three thousand eight hundred and twelve deaths reported from disease designated under the general title of cholera, among persons of all ages—cholera morbus or cholera infantum. Of this number three thousand three hundred and eight were under the age of two years; all of which were doubtless the *cholera of infants*, as that term is used to designate a specific disease.*

In the city of New York of fifteen hundred and twenty-five deaths from this cause in 1854, thirteen hundred and sixty-four were under the age of two years, and of three thousand one hundred and sixty deaths which occurred in three years, two thousand seven hundred and forty-seven were under that age.

The time, therefore, at which the disease is at its height is that of the period of second infancy, when important physiological changes occur, manifested by the appearance of the teeth, the development and activity of the salivary glands, together

* See Appendix, I.

with alterations in all the organs of digestion; a state of system which, it will be seen, when considering the pathology of the disease, is inseparably connected with its development.

Observation 5th.—*Inappropriate Food.*—This is an occasional exciting cause, acting with those already mentioned. Where a child is artificially fed, there are often indigestible substances of a farinaceous or starchy nature given, that greatly disorder the digestive organs, from the impossibility of assimilating them; these organs being unfitted for the reception of vegetable aliments. There is another form of food, which does not appear to have attracted the notice of writers on cholera infantum, but which is of importance when considered in connection with the causes already mentioned. It is the deterioration of the mother's milk from the debilitating effects of the atmospheric influences already considered. The mother suffers as well as the child, and it appears quickly in the secretion of the milk, manifested principally in its lessened quantity, while the process of nursing is one of fatigue and exhaustion. Even if it is difficult to prove any alteration in the chemical qualities of the milk, yet the fact is well established that the milk will undergo changes at times of such a nature as to impart injury to the child. We have proved this by withdrawing the child from the breast, although in general opposed to weaning children that are sick.

General Phenomena.

At the present stage of the investigation we shall state the symptoms in a general way, and those only which impart a character to the disease, and from which it derives its distinctive name, and not those which are the result of its varied forms of manifestation or of its complications; the object being to connect the signs of derangement which distinguish this disease from every other, with the causes which produce them. The statement of the phenomena in detail will be reserved for a

section on the semiology, immediately preceding that devoted to the treatment.

The disease is characterized by inordinate evacuation from the bowels, generally accompanied by vomiting. Whatever be the different modes of its invasion, whether with or without febrile action or the existence of nausea, diarrhœa is usually the first symptom. The simultaneous existence of vomiting and purging at the beginning of the disease indicates its severity; and under such a mode of attack, it will often terminate fatally in a few days. In the greatest number of instances, the diarrhœa alone is its earliest manifestation, at first consisting of the ordinary fæcal matters, changing soon, however, to a serous fluid of different hues. At intervals the discharge consists of the mucous secretion of the intestines, mixed with undigested food. The odor differs from ordinary fæces; sometimes, when it contains the ingesta, it is indescribably offensive. At other times it is in a state of fermentation which imparts an acid odor, not unlike that of the chyle before it has reached that part of the primæ viæ where the biliary duct opens into the intestines, it being diluted only with the follicular secretion. When the disease is fully established, the absence of bile is one of its distinctive marks, while the discharges indicate ulceration. Vomiting, or attempts at vomiting, is another prominent symptom, and is one of the most uncontrollable and obstinate of all the phenomena. The discharges by this act never consist of any thing more than the food or drinks that have been given. All these symptoms are accompanied by inordinate thirst.

Next appear a dry and flabby state of the skin, with intense heat on that covering the abdomen. Rapid emaciation follows, and the body becomes attenuated to a most frightful extent. As the disease advances toward a fatal termination, the natural stools are retained, and the mucus becomes gelatinous, and often pink-colored. The disease usually lasts a month or six weeks.

Lastly, evidences of cerebral disturbances show themselves, with occasional convulsions. These and other compli-

cations, being additions to the essential symptoms of the disease, are not here particularly noted, although constituting a necessary part of the ultimate development.

Morbid Anatomy.

In the different parts of the intestinal tube there are found numerous granules having at their external surface a minute pore, from which the mucus secreted by the gland is exuded. They are known as the mucous glands or granules, and also as the muciparous follicles. Although they are very numerous in the upper part of the intestines, yet they exist detached from each other; while from the jejunum downwards they appear in groups. But one function is discharged by them all, that of furnishing mucus to the surface of the intestines, although they are distinguished by different names in different parts of the intestines. They will be considered in this Essay with reference only to the functions they perform, and denominated mucous follicles.

These follicles rarely make their appearance until about the time of teething, when a general development of all the parts connected with the asimilating process takes place. When the teeth appear, the mucous follicles are brought into action, and are apparently connected with the necessity of a change of food for the child.

In cholera infantum the most remarkable anatomical change is found in the excessive development, the functional activity, inflammation, and occasional ulceration, of these follicles.

Instead of pursuing an elaborate detail of the appearances presented on postmortem examination in every organ and cavity of the body, many of which are secondary and the effects of complications, we will refer to two lithographic representations of the actual anatomical changes in parts which are primarily affected, and which continue to be the principal parts diseased, whatever modifications the disease may assume; the symptoms of the local derangement which

characterize the affection always being aggravated the nearer the approach to a fatal termination. As the fairest method also of ascertaining the morbid changes, we propose also, to exhibit an analysis of all the postmortem examinations that have been recorded, so far as we have been enabled to find them in the different medical periodicals.

Anatomical Illustrations.—No 1. The colon of a child, aged 6 months, that died of cholera infantum. The mucous follicles were all enlarged, but scarcely any of them were ulcerated—the whole mucous surface appeared to be covered with white elevated spots.

No 2. The colon of a child, aged 1 year and 8 months, that died of cholera infantum. A large number of the glands were not only enlarged, but ulcerated; indicated in the accompanying representation by the central dark spot.

Anatomical Analysis.—On examining most of the medical periodicals which have been published within the past fifty years, we have found recorded the postmortem examination of thirty cases of cholera infantum.* Of this number there were twenty-eight reported as presenting the liver diseased, either by being congested, enlarged, or altered either in color or texture. Its color was in many instances lighter than usual, and in others "variega ted." It was in most instances firmer than is the case in health. Dr. Horner observes that the liver is generally enlarged and of a more firm and solid texture than is the case in the natural, healthy condition. Only in two cases, the liver was reported healthy, or, as it is termed in one instance, "appeared healthy."

The next fact to be noticed is, that in the whole number, the mucous membrane of the intestines was found diseased, inflamed with mucous exudation, congested, or ulcerated. These statements were made before Dr. Horner published the

* By Drs. Jackson, N. E. Jour. Med., v. 1; Horner, Am. J. Med. Sci. v. 20; Stuart, Coxe's Med. Mis. v. 3; Baxter, N. Y. Med. and Phys. Jour. vi. N. S.; Lindsley, Am. Jour. Med. Sci. v. 24; Hallowell, Am. Jour. Med. Sci. v. 40.

No. 1.

No. 2.

results of his examinations. He observed the same condition of the mucous membrane; but on removing the mucus and blood by a little maceration, he was enabled to note the true organic changes that had occurred, by discovering the delicate prominences of thousands of inordinately developed mucous follicles—a large number of them in a state of ulceration. In the examination of two cases by Dr. Baxter and twelve by Dr. Hallowell, the same condition was found, and which is also represented in the lithographs accompanying this Essay.

The disease, therefore, exists mainly in the mucous follicles and in the liver. The effusions and congestions in the brain and other organs are clearly complications of the original disease; and although exhibiting serious affections of those parts, yet these organs do not, so far as we have been able to ascertain, present any decided alteration in the structure itself, as is found in the liver and mucous surface of the intestines.

Pathology.

The pathology of a disease should be studied closely in connection with the causes which produce the morbid changes exhibited on postmortem examinations. It is evident that an anatomical investigation can give us only the morbid results of diseased action, and is therefore to be regarded as but one extremity of the continuation of a morbid condition of parts; the other exists inscrutable to us, except in the symptoms which characterize it, and in the immediate results of the causes in operation to effect the changes which produce these symptoms. While the anatomical alterations show us the terminations, we must seek for something else anterior to them for instruction as to the actual condition of the living parts at the commencement, and thus endeavor to ascertain what it is that leads to these changes. Often in the examination of the philosophy of disease, we are obliged to be contented with learning from the symptoms during life, and the anatomical condition after death, on what to found its pathol-

ogy. If it be possible to add to them the known causes, and if these are not only clearly established, but are found to be peculiar in their immediate effects, we obtain a starting point of great importance, which if carefully studied and the course indicated by them be as carefully pursued, will add much to our ability to interpret the revelations of the scalpel.

As cholera infantum can be referred to well-ascertained causes, and as these agents are in action to maintain the disease, when established, by their constant and positive influences, there can scarcely be found a disease that can so well elucidate the importance of endeavoring to establish the connection between the early morbid action and the ultimate morbid results.

It is for this reason that the causes already exhibited will be taken up in their order, their philosophy examined, and the morbid conditions produced by them studied in connection with the alterations revealed after death.

1*st. Heat.*—The effects of atmospheric heat on the system are direct and indirect. By the direct effects are to be understood those sudden disorders which are the results of an accelerated action of the circulatory organs, or the more rapid exhaustion of the nervous power. This sudden invasion of disease during the intense heat of summer in temperate regions, and in all hot countries, among those not accustomed to them, is well known. The consideration of this class not being a proper subject for this paper, it will of necessity be passed over; while the indirect effects, or such as are less prompt in their manifestation, as in the disease now before us, will form the proper subject for investigation.

There is one organ which exhibits before all others the effects of heat, and that to a remarkable extent. From the earliest period the liver has been regarded as the one that suffers most from this cause; and a host of writers on the influence of tropical climates and their diseases, refer to atmospheric heat as the principal source of hepatic affections.

Such affections prevail to so great an extent in hot climates that those who have practiced in temperate regions

alone, will find it difficult to believe in their extensive prevalence, among natives as well as among strangers. Although there may be auxiliary causes, yet it is by the most accurate observers attributed to atmospheric heat as the principal cause. Thomas, Johnson, Moseley, Saunders, Larrey, Fitzpatrick and others, who have resided in hot countries, refer to heat as the agent to which the alterations in the action and organization of the liver are to be attributed. There is no other organ on which caloric appears to be concentrated in its effects, and none whereon the consequences are so marked.*

In some instances an active condition of this viscus is produced, marked at times by an inordinate secretion of bile; in many of them a want of action ensues, following this excessive action as a state of collapse follows that of inordinate excitement.

Among the effects of heat on the liver is that which is denominated chronic inflammation. This term appears to have been applied to various affections of this organ, which differ from the acute or active inflammation. A score of morbid conditions are referred to chronic inflammation. These morbid states, it is plain, may differ very essentially from chronic inflammation, and yet collectively they have been so denominated. Among these derangements is found a simply sluggish or congested state; when it becomes slowly indurated, with or without enlargement, and when it no longer performs its office of secretion of bile. This is a very common condition in the East and West Indies; a torpid condition of the liver and a paucity of its biliary secretion, are very frequent disorders in those places, attended with constipation and indigestion.

These are the consequences of disease protracted for years; but in affections of this organ arising during a shorter period, alterations less marked are to be looked for; and such are revealed in cholera infantum.

* See Appendix, J.

As heat exhibits its action on the hepatic system, so cold shows itself upon the respiratory organs, less extensively; but it is much more decided in its manifestations. Affections of the liver are more or less obscure; as this organ is less under our cognizance, both in its condition of health and of disease, little evidences of its diseased condition exist, and obscurity and uncertainty often attend its derangements, both in the young and the adult.

Summer and winter show their effects very promptly on the tender and susceptible system of the infant; the winter not among so many individuals, partly perhaps on account of the ability of most people to shield themselves and their offspring from the extreme cold, while from the intense heat of summer there is no escape.

Infants yield quickly to the influences of cold, and suffer greatly from affections of the respiratory organs during the winter; while there is no marked disease among them during the excessive heat of summer, unless it be derangements of the chylopoetic viscera. The "summer-complaint" is the popular name for one form of these derangements, from its extreme prevalence during the existence of a high range of temperature. In the absence of any facts to show that heat will produce a decided effect, among a large number of individuals, on any other part of the system than the hepatic, it would seem to be but reasonable to suppose that the gland which is so generally affected by it would not escape in the infant, but would be the part that would be primarily diseased under the circumstances which produce so violent a disorder as cholera infantum, or "summer-complaint."

The first effect of heat upon the system is through the lungs, and by interfering with the proper discharge of the respiratory function; this act being the introduction of oxygen during inspiration, and the ejection of carbonic acid during expiration. It has been proved by experiments on small animals, that a much less amount of atmospheric air is decomposed during hot than during cold weather. In an expe-

riment by Crawford, it was found that a guinea pig under a temperature of 55° consumed twice the quantity of oxygen than when confined to a temperature of 104°.

The connection of the process of respiration with affections of the liver, will be considered when the effects of the next two causes are examined.

2*d. Excessive atmospheric humidity.*—This is one of the principal causes of the extreme languor in hot and crowded places. One of the modes by which extreme moisture, with heat, acts unfavorably upon the body, is by preventing the ordinary amount of exhalation from the lungs. This is most decidedly seen when the excess of moisture arises from great crowding, and the usual domestic occupations in houses where several families reside. The houses of the poor are loaded with vapor from these causes; the temperature at which moisture is deposited scarcely differing from that of the air. We have tried the experiment with a wet bulb thermometer where the temperature was at 90°, and could not make the dew-point lower than 88°. It has been probably often at the point of saturation, when they will both range at the same degree.

The air always contains moisture, and in proportion to the amount is the quantity of moisture exhaled from the lungs. Thus, air containing moisture that is deposited at 38°, the average in this climate, will allow of a much greater evaporation from the lungs than when the temperature of the "dew-point" is at 70°.

It is shown by the experiments of Dalton that the quantity of vapor contained in a cubic inch of air when the dew-point is at 38°, is .00166852 grain; when at 94° the amount is .00938243 grain. The last mentioned is the dew-point of the vapor as it is expelled from the lungs, whatever may be the temperature of the surrounding air; and this is the amount of vapor contained in a cubic inch of air as it passes from the lungs of an adult. If the air before inspiration be overloaded with vapor, it is obvious that it must prevent to a certain extent the depuration from the lungs. We have found,

however, that unless the amount of vapor in the air very nearly approaches to that passed from the lungs, no very marked effects are produced, but a sense of oppression and suffocation arises when it approaches that point.

The weight of a cubic inch of vapor at 88° is .00789288 gr. which is .00622436 more than the mean of the climate, and .00158153 less than the amount contained in the pulmonary vapor. If these be multiplied by 1,152,000, the number of cubic inches inhaled in a day by an adult, according to Thompson, Minzies, and others, the result will be respectively 7170 and 1811 grains; the difference between the two–5359 grains–will give the lessened amount of transpiration of pulmonary vapor. When at 70°, the highest in this climate, the weight of a cubic inch of vapor is .00461639; if this is subtracted from .00938243, which is the quantity in a cubic inch of respired air, it will leave .00476604, which multiplied by 1,152,000 will give 5490½ grains of water evaporated from the lungs when the air is inhaled at a dew-point of 70°. These statements will show how much an excessively high dew-point must interfere with a proper depuration from the lungs.

Another source of embarrassment to the full healthy depuration from the lungs is the increased elastic force of vapor at a high temperature. According to Dalton, this power at 38°, is .264, and at 88° 1.286, in inches of mercury.

There are, therefore, two obstacles to a free pulmonary exhalation in an inordinately moist state of the air.

The lessening also of the transpiration from the skin, from the same causes, has an important influence upon the healthy action of the body, as is known by daily experience; but the arrest of cutaneous depuration by means of excessive moisture of the surrounding air differs in its effects from that produced by cold.

The principal results of an arrest of the ordinary pulmonary depuration is to cause some movement of compensation in another part; the consideration of which will be deferred until the next subject has been investigated.

3d. Malaria.—There are two alterations in the air when-

ever there exists any deterioration; one is the chemical change, and the other, a simple mixture of foreign and deleterious ingredients. In all crowded places the chemical change must be principally that of an excess of carbonic acid from the constant respiration. This arises from the process of respiration being that by which oxygen is introduced into the system through the blood, and thereby causing in the ultimate tissues the combustion of carbon, which escapes from the lungs as carbonic acid.

According to the experiments of MM. Dulong and Despretz, the quantity of carbonic acid thus produced is the same as would result from the combustion of an amount of carbon equal to that of which the body is deprived by the act of expiration; the carbonic acid passing into the surrounding air, and taking the place of the oxygen which is removed by inspiration. By respiration alone, an adult will transform into carbonic acid in the space of an hour, all the oxygen contained in twenty-five gallons of atmospheric air; which in a house, and especially in a sleeping apartment, is a matter of great importance.

From this process, also, arises the increased heat of a crowded room; the union of the carbon and oxygen producing an additional heat, which is added to the atmospheric temperature already existing,—the body of every animal being a constant generator of heat. The same changes occur outside of our dwellings, but are less injurious, from being more easily diffused and dissipated.

Another change in the surrounding air is produced by transpiration both from the skin and lungs. It is a mixture solely, and is independent of any chemical change. This addition eludes altogether chemical tests, and exists, it would seem, only as suspended in the atmosphere, and is conveyed by it as a foreign body, or rather by the vapor that is always present in the air. The transparent fluid exhaled from the lungs is in every respect the same; the taste is saltish, the color peculiar, and it becomes insupportably offensive when

kept in a warm place; showing that it consists of animal matter in a state of decomposition.

The perspiration is sometimes differently scented; it has had the odor of musk, violets, sulphur; and it has also been extremely fœtid and occasionally acid. It has also been variously colored,—red, saffron, black, or blue.

These facts would appear to show that the substances transpired possess properties derived from the body, and ejected as not proper to be retained. This transpiring process is indeed one of depuration, and to the extent of twenty-eight ounces in twenty-four hours. It must be, therefore, injurious to be receiving into the body by inspiration that which its necessities have previously required to be removed. Who but infants would be the greatest sufferers? a class among which the effects of external influences, of whatever kind, are so promptly experienced, and to which they so promptly yield.

All the conditions of the atmosphere which have been considered, directly interfere with the ultimate object of respiration, and when they are all present in a marked degree, the effects will sometimes appear in the embarrassment of the act itself.

When any organ is disturbed in the exercise of its appropriate functions, and a morbid action arises, the system struggles, as it were, for relief; and under this derangement, some other will be found either to be involved in the derangement, or to have assumed its functions, and thereby to have relieved the morbid condition of that one which was primarily affected.

This is of so ordinary occurrence that when a morbid functional change arises, we naturally look for a vicarious movement, which either affords relief, or by too great a demand on the energies of the body for that purpose, passes into a diseased action in a part of the system distant from that originally affected. This movement of compensation is familiarly illustrated in the reciprocal affections of the skin and kidneys, skin and lungs, etc., in a low temperature.

That between the lungs and the liver is no less remarkable, and equally important in its results.

When the air becomes unfit for the necessary renovation of the blood through the medium of the lungs, or when the usual amount of depuration from them is prevented, their functions are but imperfectly performed, and the requirements of the system inadequately supplied. Under this condition, especially when the interference is slight, but is at the same time protracted, no sudden evidence of derangement presents itself, but a decided action will be established in a part that evidently bears a reciprocal relation to the lungs. This part appears to be the liver, for the following reasons:

According to the experiments of Tiedemann and Gmelin, it has been found that the quantity of venous blood sent to the liver increases as the pulmonary system is less perfect. In the mammalia the vena portæ conveys blood from the intestines alone ; in reptiles it is also brought from the posterior extremities, tail, kidneys, and other parts. In connection with this, is the fact that the temporary suspension of the functions of the lungs, and the consequent arrest of the flow of blood through them, in hybernating animals during their period of sleep, is not attended by a suspension of the functions of the liver, bile is secreted as usual; corresponding with the human fœtus, where the total inactivity of the lungs appears to be compensated by some action of the liver.

The vicarious action of the liver, is also manifest in the pathological condition of that organ from the effects on the respiratory process. Some of them occur in malarial districts, and are well known to residents of those places. Hepatic diseases affect hogs and sheep that are exposed to paludal effluvia, while "gall sickness" is a popular designation for similar disorders among men. One of these results, which exhibit in a strong light the connection of the morbid derangement of the lungs and liver from malaria, is the complication of pneumonia with hepatic disease, known as bilious pneumonia.

Another illustration of the vicarious action of the liver,

when the functions of the lungs are impaired, appears in the fact that fatty livers are frequent in pulmonary phthisis, of which M. Louis and Dr. Horner give several examples. The former mentions forty-nine cases, of which number forty-seven were dependent on that affection. Fat, it is well known, abounds in carbon.

That morbid hepatic affections will arise from breathing in a confined atmosphere, where the lungs are forced to receive air deprived of a portion of its oxygen, appears from the results that have followed experiments on animals purposely confined where the air could not be freely renewed. Dr. Baron placed several young rabbits where they could not obtain fresh air, feeding them on cabbage and grass. One of them died in a month, and presented a morbid condition of the liver, consisting principally of vesicles over its external surface. In nine days another died, with tubercles in the same viscus. In four days another died, with the whole of the liver diseased; and in two days another died, exhibiting the liver in the same condition. The three that remained were removed to an open situation, and recovered.

Physiology, and the comparative anatomy of this organ, support the opinion that it does perform a function supplementary to that of the lungs, and that, with respect to the venous blood that enters it by the portal vein, it is an organ for depuration, being the only one in the body in connection with the lungs, if we may except the skin, that acts in removing the carbonic acid from the system. The skin, however, is very limited in its excretory function in this particular.

That which appears to be the most prominent fact when considering the external causes of cholera infantum is, that they all are connected with some interference with the proper discharge of the functions peculiar to the lungs, and consequently with an imperfect removal of carbonic acid from the system.

The importance of this depurative process is evident from the physiological history of all animals; they all require oxygen, and the effect of its use is uniformly the presence of

carbonic acid in the air, with which they are surrounded, when the circumstances are such as to prevent the results of respiration from being dissipated.

It cannot be supposed that whenever so general a law of animal nature is interfered with, the system would retain its healthy vigor, and discharge with suitable energy its varied functions. It would seem as if the retention of carbonic acid is the retention of an effete substance possessing peculiarly injurious qualities, and which, by the action of some of the organs, must be removed. So virulent, indeed, is the poison, that if all the oxygen is removed, so as to preclude the possibility of its action on the body, speedy death ensues; if a less quantity than is required be supplied, uneasiness, debility, and fainting follow. In an atmosphere containing more, but still less than would satisfy the requirements of the body, a protracted derangement of one or more of the organs is the result.

Now, the lungs perform the function of relieving the system from this pernicious ingredient; and all the causes which have been considered do, each in its own way, interfere with this process. Thus, it has been proved by direct experiment on small animals, that a lessened consumption of oxygen occurred as the temperature increased, and that a difference of 49° made a difference of one half of the amount consumed. Excessive humidity added to this high temperature, also interferes with the proper action of the lungs, by preventing a full exhalation. Malaria, especially in crowded places, also interferes with the function of respiration, by supplying air deficient in oxygen, and charged also with foreign ingredients. All these causes of the disease unite in interfering with the normal results of respiration, either by rendering the air deficient in what may be denominated the vital ingredient, or by some physical modification which prevents a sufficient quantity from being inhaled.

There being no proof that the active causes of cholera infantum directly affect the mucous membrane of the intestines, but abundant evidence that they do produce a morbid

change in the liver, this organ is probably the first of the abdominal viscera that is affected in the disease, by the establishment of a vicarious action; the lungs not freely relieving the system, and the liver being a supplementary organ performing a part of their functions.

The transition from an over-excited state of this organ to one of collapse or congestion, and to other morbid conditions, is a subject familiar to every medical practitioner, and requires no illustration.

The morbid state of the mucous surface of the intestines follows next, being the result of the morbid condition of the liver, as will appear when the influences of the fourth cause are considered.

4th. Period of Life.—This may be regarded as the predisposing cause, for the disease never appears but during the period of teething. When children that have passed that epoch are exposed to the causes already considered, the result is dysentery. This the writer has repeatedly seen in the same house, during a hot season, where cholera infantum has prevailed. When the affection of the bowels that was excited in the older children continued for a few days, it always assumed the form of dysentery, but never that of the peculiar disease of infancy.

When the morbid condition of the intestines was stated, it was shown that the source of the flow was the mucous follicles that studded the greater part of the intestinal surface, and that these follicles presented an appearance of inordinate development, inflammation, and ulceration. Their development is peculiar to the period of life at which cholera infantum appears, is the result of the natural movement of the system, and is simultaneous with the eruption of the teeth. The inflammation and ulceration of the follicles are superadded morbid action, constituting the disease under consideration.

In the infant and child, the active process of development is an important predisposing cause of disease. In them everything predominates that is connected with organic growth. The desire for food is frequent, the digestion rapid,

the blood and fluids are in excess; capillary action, secretion, and interstitial increase proceed with great vigor; while the nervous sensibilities are in high activity. Growth of parts seems in truth to differ from inflammation only in degree, which may very speedily be induced upon any disturbance of the developing process.

It has been remarked that infants grow more during the first year, the increase becoming less rapid as the child approaches the fourth or fifth year; and it is well known that during the first-mentioned period, violent disease and sudden mortality more frequently occur than at any other time of life. Here, then, is a direct relation subsisting between the rapid increase of the body and its tendency to severe and fatal disease; the naturally exalted action of the capillaries in developing a part being easily made to pass the boundaries of the healthy process of growth, and become the active agents of inflammation; the effects of which are of marked peculiarity,—prompt serous effusions from some membranes, and in others the exudation of lymph, as in the trachea in inflammations of that tube.

The diseases of the brain furnish an evidence of the transition from health to disease during the process of growth; it is well known that at the time of birth the cerebral organ, although large, is exceedingly imperfect in its organization, consisting of a mass almost like jelly. It is not complete in its parts until a year or more has elapsed, during which period the vital energy of the part is in great activity, carrying on the process of development. Congestions, effusions, and convulsions, are more common at this time than at any other.

In other instances there is an excess of action in one set of organs, which gives a preponderance of one system over others, as in the lymphatic; producing congestions and obstructions in the different glands of the body.

Another illustration of the effect of the development of parts in the production of disease, is that afforded by the mucous follicles of the intestines. When, in the order of the natural development of the infant's frame, these follicles for

the first time show themselves in connection with other parts concerned in nutrition, they are in a high state of activity, pouring forth an abundance of their natural secretion. Thus far the action is a natural one, and one of health. Should it become excessive, a serous diarrhœa takes its place, demanding the interference of the physician for its removal. This same condition of the development of the follicles, when complicated with other derangements of the system, and kept in a state of morbid activity by the continual operation of certain exciting causes, terminates in producing one of the most fatal diseases of our climate, the cholera of infants.

Dissections of infants that have died of various diseases, have exhibited the development of the mucous follicles, commencing at the time of the appearance of the teeth, while before that time they are rarely to be seen. Numerous post-mortem examinations* show that the follicles experience an increase of vital energy which augments their secretion, and renders their size larger and their number greater, but which still does not produce any redness, tumefaction, or ulceration. Their activity in secretion during the time of dentition is the true cause of the ordinary diarrhœa in teething infants, usually ascribed to sympathy with the gums, and which to a moderate degree is not to be regarded as disease, but is the result of the physiological action of the period of life.

The follicles, in the natural course of development, having just passed into a state of activity, are thereby prepared to have an additional or excessive development, on the application of a sufficient cause; and then the transition is both rapid and easy from a healthy to a diseased state.

The normal follicular action passing into an abnormal state, under the influence of the three atmospheric causes of the disease, is effected through the agency of the liver; which, we have seen, is the part on which all these causes exercise their power. They produce disease and alterations in its density or texture, and thereby the passage of blood that

* Billard, Malad. des Enf., p. 306.

is returned through it from the intestinal surface is obstructed. Congestion of the mucous membrane and its follicles takes place, and nature seeks relief by an inordinate secretion from them, already in an exalted state of action. When morbid action is once established, congestion, inflammation, or ulceration is the course naturally to be expected.

It appears, therefore, that cholera infantum is a disease connected with the development of the body, and is the most remarkable instance of the relation subsisting between the growth of parts and the diseases of childhood and infancy. The effect of growth shows itself in the violence of inflammatory diseases, as connected with the exalted state of interstitial nutrition, so characteristically rapid at an early period of life. At one time, it is exhibited, as we have seen, in a peculiar exudation from the blood, abounding in fibrin, as in croup; again, in the morbid action connected with the changes in the cerebral organ; at another time, in the succession of diseased action of the different parts of the glandular system; and in the disease now under investigation, in the morbid development of the mucous follicles of the intestines during the period of second infancy.

5*th. Inappropriate food.*—There is little to be said upon the action of food that is digested with difficulty. The portion not disposed of by the action of digestion becomes, it is well known, an irritating body, which, of course, must irritate all the parts through which it passes. That kind of food which is the most quickly absorbed is least likely to add to the causes already in operation to develop the disease, which must be promoted by intestinal indigestion.

Semeiology.

The division of diseases into different stages, is both natural and useful. Like all other natural phenomena, disease has its peculiar signs in its incipiency, maturity and decline; exhibiting phases which indicate the kind of interference which it is necessary to adopt in its management. Without

availing ourselves of these as a guide, the practical employment of remedies would be equally unscientific and unsafe. The boundaries, however, of the different stages do not always appear so distinct as to enable us to detect clearly the progress of the disease from one to the other, and the complications add their influence in rendering the actual knowledge of the condition of the affected parts obscure, and thereby often tend to embarrass the judgment.

Cholera infantum, in its progress, appears in three different conditions or stages. The first is marked by an increased activity of the bowels, the second by inflammation and ulceration, and the third by complications.

The evacuation in the first stage consists, at the beginning, of the usual fæcal matters, more largely mixed with watery fluid than in health. Indeed when the disease is gradual in its invasion, no difference will be observed between it at the early stage and the diarrhœa of teething infants, which always precedes cholera infantum, and is in truth its commencement. The discharges consist largely of serum, mixed with mucus or minute shreds, or colored with bile; but in all its forms it is characterized by copious watery evacuations, which, at the period of teething, are often so abundant and debilitating, that the child is in a few days exceedingly emaciated and exhausted. This is probably the form that is known as the "watery gripes" in England, when it has not passed into the decided morbid condition which will be more particularly noticed as the second stage.

To a moderate extent, diarrhœa at the time of teething is salutary; should it, however, increase in severity, and exhibit a diminution of bile, we may regard this increased action of the mucous follicles as evidence of the congestion of the mucous surface of the intestines, from some degree of congestion of the liver; the morbid action of both these parts being the pathological condition of cholera infantum.

Although for the most part serous, the consistency of the passages will often differ in the course of the day,—sometimes thick and pasty, or frothy, at other times bearing a resem-

blance to the yolk of a hard-boiled egg beaten up with water.

This diarrhœa affects the system both by the debilitating effects of the increased peristaltic action and by the prostration occasioned by the great secretion from the mucous surface.

In this diarrhœa the color of the discharges will be different, not only in different individuals, but also various in the same patient at different times of the day—yellowish, light green, or of the peculiar color of serum, and equally as transparent. This last condition we have frequently seen, and so destitute of any additional substances, as to have been mistaken by the nurse for the urinary discharge, and the fact mentioned of the excessive amount of urine passed by the child, when in truth there had been none.

It is rare that vomiting occurs very early in the disease; yet, when it arises simultaneously with the first appearance of excessive purging, the disease is proportionately severe, and exhaustion and death will often ensue, without any complication, in a very short time. We have witnessed death under these circumstances in twelve hours. Such instances, however, are rare, and it is unusual for a fatal termination to occur during the first stage.

The vomiting, or efforts at vomiting, is a marked feature of the disease. All observers must have noticed its obstinacy and the peculiar kind of action attendant on the attempts at vomiting. This effort is sometimes connected with a powerful action of the abdominal muscles; and they will be found at times, on placing the hand over the abdomen, to be in a convulsive or rolling motion, generally simultaneous with the action of the stomach. At other times when the disease is severe, this movement of the muscles will take place without any attempts at ejecting any thing until the paroxysm is about ending, when the efforts to vomit will occur.

The tongue is moist, and covered with fur of a light brown hue at its base. The thirst is invariably very great.

The febrile condition does not arise from an essential or

primary fever, but is clearly of the class of irritative fevers; it rarely appears at the beginning, but increases with the progress of the disease, occurring in irregular paroxysms of different degrees of intensity. The heat of the surface of the body augments in the course of the disease, especially in the abdominal region.

The pulse is small, quick, and frequent, sometimes sharp and wiry.

The second stage is the period of inflammation and ulceration. The general heat of the body decreases, while that of the abdominal surface increases to a high degree of intensity.

The tongue is parched and heavily laden with petechiæ at the sides and at the commissure of the lips. The child seizes with frantic eagerness any drink that may be offered.

The serous diarrhœa increases, with great variety in the appearance of the evacuations, and with the addition of mucus. While before, the predominating color was yellow, it is now frequently of various hues of green. At times the serum appears to be replaced by masses of mucus, of a greenish tinge to a deep hue of that color. Although a yellow or green color may indicate the presence of bile, yet the alvine evacuations may, in disease, assume this appearance without the presence of bile. The jelly-like mucus derives its color from the alterations in the secretion produced by inflammation, as is occasionally found in mucous discharges in disease in other parts of the body. In bilious evacuations, there is a decidedly fæcal odor; in the colored mucus, none. The color is also changed to a brown or white, and frequently pink. During their evacuation the usual fæcal matter is retained.

These discharges are at times peculiarly offensive, at other times they exhibit no other odor than that which is derived from acid. The offensive odor is not that of ordinary fæces, but of the nature which attends the decompositions of animal tissues in ulceration and sloughing

The evacuations present different appearances, according to the part of the intestines the most affected; thus when the upper part is that principally diseased, they are frothy and

acid, and when the food is passed, it appears almost unaltered by the process of digestion. When the lowest portion of the intestines is the part, the disease bears a resemblance to dysentery, and the evacuations are bloody and slimy, and attended with tenesmus and pain.

In inflammatory affections of the bowels in children, there is always some sympathetic affection of the brain; and serious disease of this organ will follow an inflammation in the digestive system; a fact well known to the ancients, but more particularly pointed out by modern observers. These pathological sympathies are in general proportionate to the violence of the primary disease, and are indeed so uniform that the meninges of the brain participate in an equal degree with the affections of the bowels, having at one time an acute and at another a chronic action. The brain, in confirmed cholera infantum, never escapes.

When the disease has proceeded thus far, the general aspect of the child is that of distress from pain in the head as well as in the abdomen, indicated by the lineaments of the face. Pain is indicated as existing in the abdomen by a pinched expression of the face, by a fold in the commissure of the lips outside of the orbicularis muscle, and by a lineament extending from this part to the inside of the alæ nasi, and sometimes surrounding the orbicularis muscle. Pain in the head is known by a frequent corrugation of the skin below the eye-brows. These most usually occur upon the awaking of the child, and will continue for a minute, or shorter period, before it evinces pain by crying. Added to these, the languor, paleness, sunken eyes, with their dark areola and general shrunken condition of the body, complete a picture of distress which it is in vain to attempt to describe.

The third stage is that of complication and death.

The skin becomes dry and ash-colored. It hangs loose, in folds, about the body, while over the forehead it is tight and smooth. The cheeks become shriveled, and the general aspect of the cheeks and lips imparts an appearance of old age, while the muscles in every part of the body are flabby, and

frightfully attenuated. The eyes are strained, and the adnata are injected; drowsiness, stupor, and partial paralysis succeed, to be followed by death after a protracted term of suffering.

Such is the usual course of the development of the disease in its progress to a fatal termination. When it does not reach the stage of complication, it is often exceedingly protracted; and when the usual secretions are to a certain extent restored, it assumes more of a mixed character, resembling a chronic diarrhœa, and is as obstinate and as difficult to manage as any disease of childhood; terminating fatally from mere exhaustion.

The discharges frequently change their character—becoming serous, mucous, flaky, bilious, and purulent, by turns. This chronic diarrhœa affects the system both by the debilitating effects of the irritation of a large diseased surface, and by the uninterrupted secretion from the mucous membrane; to which may be added the almost total loss of the power of digestion. Whatever is the character of the alvine evacuations, a want of tone in the affected parts appears to be their pathological condition, with occasional ulceration of them.

Treatment.

In the treatment, it must be kept in mind that the use of therapeutic means is of little avail while the active agents in the production of the disease are in continued action upon the system. Often have we experienced how disheartening it is to be required to administer medicine for the cure of this disease, when the circumstances of the patient are such as to preclude the possibility of applying to any extent the measures demanded by the laws of our physical nature. Even the temporary relief sought during the day is productive of little benefit by a return at night to the close and stifling air of the over-crowded dwelling.

To exhibit by facts in as strong a light as is in our power,

the important influence exerted by a pure atmosphere in overcoming the morbid action in this disease, we will give the results of several cases; those only of a large number, occurring both in public and private practice, of which we have any memoranda. The circumstances under which they happened were so marked, as to admit of their being divided into three classes.

Of ninety-four, forty-four occurred where the concentrated effluvia of about five times more people than ought to have occupied the space of their dwellings, existed, in addition to the refuse produced by them. The odor of the air was at all times distinctly offensive to the senses amidst the stifling heat of the weather. Of these forty-four, twenty died. The next class occurred among persons possessing the ordinary comforts of such as usually apply at the dispensaries, and were about half as much crowded as those already mentioned; they numbered thirty-five; of whom ten died. The last consisted of private patients, and numbered fifteen; of these, two died.

Could we add to these numbers, it would, without doubt, add an abundant testimony in corroboration of the prevailing opinion of the necessity of pure air as a curative agent.

Both as a preventive, and as a remedy when the disease exists, it will be necessary to adopt suitable means to modify the excessive heat and to purify the vitiated air. It is indeed difficult, if not impossible, among such as are exposed to the sufferings and have to endure the privations of poverty, to accomplish any thing of this nature. An attempt, however, may be made by exposing the child daily to the reviving influences of the fresh air, by a walk in the suburbs, or if the situation permit, by an excursion on the water. The causes of the disease are clearly obvious; and these measures it is the duty of all physicians to urge, during the intense heat of summer, that nothing may be omitted in the way of preventing so formidable and fatal a scourge.

It is surprising how often is witnessed among the more intelligent, who are removed from the effects of poverty, an

entire disregard of the simplest rules of hygiene. Frequently we have seen young children suffering from the effects of heat and impure air, languid and sweating, from being crowded in one bed or in one room while the external air is carefully excluded. Placing children to sleep in a low bedstead that has been kept throughout the day beneath another, and only brought out at night, is fraught with the direst of evils, never thought of by those having the immediate care of the children.

There is no difficulty among those whose condition in life enables them to use the simple prophylactic means necessary, to anticipate this and other affections of children that arise from heat and impure air. The doors of all the bedrooms should be left open, while the upper sash of the window should be let down; no danger can arise from the external air admitted in this way, if the bed is removed from the direct current. During the day, in summer, the freest circulation ought to be encouraged by keeping opposite windows open.

Children should never sleep on a feather bed: a hard matrass that will not accumulate the heat is the only kind of bed they should use; while the body ought to be lightly covered.

The prostrating effects of heat in summer are particularly distressing to young children, in whom the activity of the nutritive process is great; and in proportion to this activity of the capillary system, is the demand for fluid; while the increased cutaneous excretion increases greatly this demand for fluid. Nothing will supply this necessity so well and be so refreshing, as a draught of cold water. All persons have experienced this desire; and the uneasiness and fretfulness of young children, who are unable to express their wants in any other way, arise very often from intense thirst, which nothing will quench but cold water. To allay this irritability, is itself of importance, and they may often be calmed at night by a drink of water. This we have always placed among the general preventive measures.

Diet is a very important subject, both in the prevention and management of the disease. One of the greatest evils in

its treatment is the use of vegetable food, either of a farinaceous or starchy nature. The last mentioned, so often given in the form of arrow-root on account of its soft mucilaginous consistency, is especially objectionable. All vegetable substances are unsuited to young infants; but those of an amylaceous nature being altogether insoluble, are with difficulty absorbed, and when the mucous membrane is diseased, scarcely ever are, but pass as foreign substances quite through the bowels, and may be detected in the discharges by iodine, the usual test for starch. Farinaceous articles are very likely to become acid, and excessively so in this disease. We regard all such articles for food in early infancy as highly pernicious, whether the child be sick or well.

The food that nature supplies is the best, and, if there is no deterioration either in quantity or quality, should be the only food given. When, for any reason, it is necessary to make a substitute for it, milk properly prepared for the child should still be given.*

In cholera infantum, we have been in the habit of combining a solution of gelatine with the milk, as the most appropriate food for the child; and, where the stomach is excessively irritable, to give nothing for a while but the jelly, prepared thick or thin, as the child will take it the most freely. The use of this substance has so frequently been followed by decided benefit, that we regard it as a valuable remedy. If gelatine comes under the class of respiratory food only, then it must be regarded as a therapeutic agent. It is so perfectly soluble that it is absorbed as easily as water, and no doubt has passed into the circulation before it could reach the diseased part; in which respect it contrasts very remarkably with arrow-root, so frequently given in this disease.

As regards the indications of a want of proper nourishing or stimulating power in the nurse's milk, we have been much in the habit of being guided by the instincts of the child, which will cause it at times eagerly to seize some animal substance and suck it with avidity. Following this suggestion,

* See Appendix, K.

we have directed cow's milk to be given, either diluted, pure, or combined with gelatine, changed occasionally for chicken water, and when the child is especially eager for animal food and has suffered much from debility, beef tea or a piece of fat pork, according to the instinct of the child. These last-mentioned articles, and even salted meat, are especially beneficial in the more advanced forms of the disease and during convalescence. In some form animal substances are necessary in every stage.

Like all other obstinate diseases, cholera infantum, is not without its specific remedies. In succession, alkalies, quinine, the stronger mineral acids, nitrate of silver, acetate of lead, oil of turpentine, etc., have been reported as invaluable in the treatment of this disease; some of them administered for the accomplishment of a definite object, and some, apparently upon no medical principles whatever. When given as specifics, they have all in their turn disappointed the practitioner, and the necessity of less reliance on the reported efficacy of some one agent, and of more upon the indications of a correct theory, becomes more evident in proportion to a more extended experience in their employment. It is by no means intended to question the accuracy of the statements made as to the success of these various articles; but the numerous failures in other hands, show clearly that the different modifications of the disease in different places, or the use of the remedy under other conditions of the system than that in which it was successfully employed, such, for instance, as a different stage of development, have doubtless been the reasons for want of success of a medicine which perhaps has been so much extolled, as to convey the idea that the remedy for the disease was at last discovered. The use of astringents is generally indispensable, but an exclusive reliance on them will certainly eventuate in failure; such remedies also as are applicable to an ulcerated condition, would be positively injurious under any other circumstances.

The success, therefore, of a specific agent is not to be referred to the power it possesses over the disease, like that of

an antidote to a poison, but to its adaptation to the morbid condition existing at the time; hence its failure under a different state of the system in the same disease.

The action of the known causes of the disease, and the morbid condition produced by them, are to be our sole guides in its management. The modifications in the general phenomena in a protracted case, the predominance of some symptom tending to divert the attention from the comprehensive view necessary always to take of the disease, and the complications which arise, will indeed tend much to embarrass the practitioner in his judgment, and to lead to a vacillating mode of treatment. But while these untoward symptoms are duly estimated and judiciously met, the main pathological condition and characteristic development of the disease must always be kept in mind, that we may not be induced to rely too much upon auxiliary remedies.

First Stage.—When the diarrhœa peculiar to teething infants becomes inordinately copious and frequent at the season when cholera infantum prevails, it is our habit to regard its probable termination in that disease. If there are evidences of acidity in the primæ viæ, it is best to neutralize it by some fixed alkali. Soda or potass appears often to exert a direct sedative influence on the gastro-intestinal mucous surface, and thereby equalizes and lessens the excessive peristaltic motion of the bowels; perhaps this effect is produced by simply neutralizing the acid which stimulates the affected part. The bicarbonate of soda combined with gum arabic and sugar,—about one-twentieth part of the alkali,—makes a good form to administer an alkali to a young infant. If this should fail to control the discharge, the carbonate of lime, in the form of chalk mixture, may be used. It sometimes happens that chalk, like other alkalies, will form with the acid in the stomach a purgative salt, and the discharges will be thereby increased. When this is the case, or if the diarrhœa is in no degree controlled, the mild vegetable astringents may be used. They may be freely employed, provided there exists no febrile action nor any heat of the abdominal surface. The

root of the common blackberry, Rubus *villosus*, or the root of geranium *maculatum*, or the bark of the white oak, Quercus *alba*,—in the proportion of half an ounce to a pint of milk, made into an infusion by boiling, is a convenient form for their administration. To an infant of six months, a teaspoonful may be given five or six times a day; a desert spoonful to a child of two years, at the same time using frictions with stimulating substances over the liver. Whenever it is attempted to arrest any copious discharge from the bowels, it is important to establish by such means some cutaneous revulsion by the use of a simple stimulating embrocation. At the same time the gums should be examined, and if found to be swollen, freely cut. Such is the mode of managing too excessive a diarrhœa from teething.

When there is reason for believing that the affection will result in cholera infantum, the discharge exhibiting less the presence of bile, frictions with mercurial ointments may be advantageously used, with or without the addition of powdered camphor, which need not be employed if any stimulating application to the skin has been previously used.

In mild forms of cholera infantum, we think the Hydrargyrum cum creta is a useful medicine, when it is deemed advisable to resort to the mercurial management. Objection has been made to it on account of its alleged uncertainty of action; this, however, can only arise from its being carelessly prepared, which may also be the case with any other compound medicine. It may be prescribed with advantage in doses of five grains every two or three hours, either alone or combined with Dover's powder and carbonate of magnesia, guided by the presence of abdominal pain or of excessive acid.

℞. Hydr. c. creta ʒ ss.
Pulv. ipecac. comp. ℈j.
Magn. carb. gr. xv.
M. Divid. in pulv. xij.

One of these powders to be given to an infant once in four or six hours. If these should completely arrest the alvine

discharges, an enema ought to be given, and at least one passage secured in twenty-four hours. If vomiting be present, the Dover's powder may be omitted.

Dover's powder is often useful early in the disease; its employment being indicated when there exists much fever and pain in the bowels. The addition of a warm bath before prostration has commenced, will soften the cuticle and cause a dilatation of the cutaneous exhalants, and a diaphoresis will, almost of necessity, follow its use.

When the disease is fully formed, mercury becomes indispensable in its management, and the best form is calomel. It should not be given in large, purging doses, which only irritate the mucous membrane, while it passes off without producing any other effect than increasing the secretion which it is one object to restrain. From the twelfth to the eighth of a grain should be administered every two, four, or six hours, intimately combined with some inert substance, as fine sugar. Medicines, it is well known, will often act with more efficiency when minutely divided; and calomel in this disease will be found to be far more effective in producing its specific action when carefully divided by being rubbed and well mixed with the purest sugar. The combination should be reduced to the finest impalpable state, and in this condition made into syrup with water. Calomel is especially useful in the first stage of the disease; at a later period, when the evacuations have become excessive, and inflammation and ulceration exist, it will pass with the evacuations, and produce no effects on the liver.

The use of opium in young children requires a great deal of cautious judgment. It is a very valuable remedy when judiciously given, and there is none so pernicious when indiscriminately used. At the commencement of the disease, it should never be employed.

One of the chief indications in the cure of the disease being to induce an action on the liver, and thereby relieve the congestion of that organ by a free secretion of bile, the calomel should be continued as the principal means of relief un-

til freshly secreted bile appears in the evacuations. Whatever be the other measures used in the course of the disease, the liver must be brought to its normal condition by this remedy. When a free circulation of blood from the intestines is secured, and the congested condition of the remote branches of the portal vein relieved, the disease then comes under control.

Such are the general measures the best adapted for the cure of cholera infantum, and are those which should be adopted in the onset of the disease, and, with occasional modifications, used throughout its course.

Some alteration may be required in the general course, or rather a suspension of the principal object may be necessary, from the appearance and persistence of some violent symptom which may itself very materially interfere with the proper use of the appropriate remedies, or become a source of severe and protracted irritation, and which it will be necessary to counteract by some special means adapted to the nature of the existing symptom.

Among the most obstinate symptoms in the first stage is vomiting, and while it continues incessantly tormenting the child, the administration of the proper remedies for the removal of the disease is of little avail; it therefore claims a large amount of the physician's attention.

For the control of this symptom a large number of remedies of the most opposite nature has been advised; all of them at times successful, and again not only useless, but frequently aggravating the symptom for the removal of which they were given.

The different anti-emetics advised in this disease are, 1st. The neutralizing, such as the alkalies. 2d. The neutralizing and astringent, as the alkaline earths in the form of lime-water or chalk mixture. 3d. Astringents; vegetable, as kino; mineral, as the acetate of lead. 4th. Anodyne; as opium. 5th. Stimulating; coffee, spirits of turpentine, ether, wine, brandy, etc. 6th. Revulsives and rubefacients, mustard, red and black pepper poultice, camphor as an embrocation, or

esicatories over the gastric region, and stimulating enemata; nd 7th. Remedies fulfilling the general indication for the ure of the disease; as mercury in various forms, either alone r in combination with others for the accomplishment of the ame end; as calomel or blue pill with Dover's powder or repared chalk. All of these have been recommended from xperience in their use, but it is plain that they could only e successful from different conditions of the stomach at the ime of their employment, and are not applicable to other ases where similar conditions do not exist.

Although vomiting, particularly when the disease is fully ormed in the first stage, may be regarded as essential to the isease, yet it is evident, from the nature of the different remdies, that some difference must exist in the pathological conition of the organ which is its seat, and which can in general e ascertained by carefully watching the symptoms attending, nd noting the state of the whole system, whether one of xcitement or exhaustion.

At the beginning of the disease there appears always to xist acidity, and we invariably begin with some alkali; limeater combined with milk rarely fails to arrest the vomiting t the expiration of two hours, when given every half-hour in he dose of one teaspoonful of the mixture, consisting of qual quantities of the two articles. Although at times readly arrested, it will recur, and will probably be arrested the econd and third time. If the disease progresses it cannot be ontrolled by this simple means, other and more powerful neasures will be required. Applications of a stimulating ind can then be used to the surface covering the stomach, ut blistering should never be employed. Blisters appear to e always positively prostrating to young infants, particularly hen applied to this part of the body. Revulsives by stimuating enemata will transfer the irritation; and if dysenteric ymptoms appear, vomiting usually ceases; it rarely occurs hen these symptoms arise early in the disease. Of the two, rritation in the rectum isto be chosen. Mildly stimulating

enemata, are important remedies in severe vomiting which resists other measures.

But it often happens that great exhaustion follows a severe and protracted vomiting, and stimulants are indicated and often receive the appellation of specifics, when it is simply the condition of the system that makes them efficacious, and whatever is the last employed is usually regarded as possessing anti-emetic properties. Strong coffee, or brandy, is among these, and under the circumstances just mentioned, highly useful in rallying the system and relieving the stomach by the general stimulation of the circulation.

Thus, by endeavoring to ascertain the actual condition, it may be in our power to control and manage to some extent this obstinate difficulty to the general management of the disease.

Instead of vomiting, tenesmus is at times the most distressing symptom, and is exceedingly wearing to the young infant. Irritation about the rectum is almost sure to relieve vomiting, and may therefore be but little interfered with; but when it passes into severe tenesmus, mild anodyne enemata should be used. They ought to be employed with great caution. The rule in the adult is, to give three times the amount of the tincture of opium by injection as it is usual to administer by the mouth. This rule is inadmissible when young children are treated. Children are peculiarly susceptible to the action of opium, although a most decided advantage often follows its use in the characteristically irritable condition of the system at this period of life; yet, from the great rapidity of absorption at that time, a small quantity cannot be given without incurring some risk. Let it be first given in an enema in the quantity which would be given by the mouth, and repeated if it should be returned, rather than increased in quantity. After a few trials, the quantity may be cautiously increased.

During the continuance of the disease, although free serous evacuations continue, the ordinary fæces are apt to be retained. This may be suspected when the abdomen becomes tumid, and the alvine evacuations have undergone no altera-

tions. If the mercurial preparations have failed to procure an evacuation of fæces, a laxative consisting of magnesia and rhubarb, will be proper to remove the highly irritating contents of the bowels. There is often acid existing in the stomach and bowels in this disease; for from the absence of bile no neutralization occurs, and dissections have exhibited the presence of a frothy, acid matter in different parts of the bowels. The combination, therefore, of some alkaline remedy will be called for by this probable condition. When there is no fever present, the aromatic syrup of rhubarb and magnesia will be found a useful prescription. The object being simply to relieve the bowels of the load of irritating fæces, such measures are indicated as will at the same time tend to produce a lessened flow of the serous discharge from the exhalent vessels of the mucous membrane.

As to the employment of astringents a great deal of caution is to be used, neither rejecting them, nor using indiscriminately such measures as will at once arrest the most prominent and exhausting symptom of the disease. Although astringent remedies are not to be regarded as the principal mode of cure, yet when the child appears to be rapidly sinking from excessive exhaustion produced by large serous evacuations, it will be advisable to employ some of those already mentioned, or others of a more powerful efficacy.

Whenever it has been deemed advisable to resort to the use of astringents, we have been in the habit of using them as auxiliary remedies, and, that their action may not be interfered with, to suspend for a short time the mercurial course. Tannin, either pure or as it occurs in kino and catechu, will often temporarily arrest the discharges. One of the most efficacious astringents is the acetate of lead, combined with Dover's powder in the proportion of the third of a grain of the former to the twelfth of a grain of the latter, to a child of fifteen months once every two hours. When the secretion of bile is established, there is nothing so well adapted to complete the cure as this combination. When using the stronger vegetable astringents, as kino or tannin, it will be

necessary to stop the animal gelatine which may have been used as diet; or it will form with them an insoluble compound, and add to the existing disease the irritation of a foreign, indigestible article.

Second Stage.—The treatment of what we have called the second stage of the disease, is that which will be best adapted to remove the inflammation and ulceration of the affected parts. If the inflammatory period is early in its invasion, and appears almost simultaneous with the appearance of the disease, a leech to the verge of the anus will be advisable. If this should not afford relief, one or two may be applied over the region of the liver, or dry cupping may be used to the same part, according to the ability of the child to bear the depleting process. It is, however, exceedingly rare that the loss of blood is required,—the inflammatory or congestive stage appearing usually after a large loss of fluids has debilitated the little patient, clearly forbidding the use of sanguinary depletion. When these symptoms appear after some days, the most that the child can bear is dry cupping in different parts of the abdomen, followed by fomentations of warm water. It is well known to practical men that instances constantly occur where there exists local inflammation, and where it would be exceedingly rash to abstract blood; this is especially the case in the disease under consideration.

In that state of the system which exhibits this affection with inflammatory action and fever, the mercury which is given may very advantageously be combined with Dover's powder. There is not much danger of exciting vomiting by its use; for, as has already been remarked, when the disease is exerting its force on the intestines with an inflammation of these parts, there is but little disturbance of the stomach. Half a grain of Dover's powder, with the sixth of a grain of calomel, may be given every four or five hours. We have thought that there is less irritation to the mucous membrane in this stage of the disease, where the blue pill is substituted for the calomel.

It is in this stage of development especially, although useful throughout the disease, that we have found gelatine, either alone or combined with milk, highly beneficial in its management. It is so soluble as to be easily and entirely absorbed, and if it supply but partial nourishment to the wasting body, or if it be altogether what is denominated respiratory food, and indirectly prevents the oxidation of the tissues, yet it is especially an unirritating substance to the tender mucous surface, differing altogether, in this respect, from any of the vegetable mucilages or jellies, which, for the most part are undissolved.

Combined with this inflammatory condition is a greater or less ulceration, exhibited in the discharges already referred to, when speaking of the symptoms. It is in this state that the nitrate of silver is beneficial, and it is probable that it is under the same circumstances that sulphuric acid or creasote has been employed with advantage. The nitrate of silver appears to be well adapted to the ulceration of the diseased parts, and when judiciously used, is a very soothing and efficacious remedy to the inflamed parts. It may be given in the quantity of one-twelfth of a grain, with about one drop of the tincture of opium, three times a day.

Third Stage.—This is the period of complication. When the cerebral symptoms, such as congestion and stupor, appear, very little if any treatment is of any use. We have often resorted to the various modes of combating congestion of this organ at this period, and have as often failed in obtaining any relief. We regard, therefore, the symptoms marking this stage as preceding a fatal termination.

The Chronic form.—When the disease is greatly protracted, it becomes mixed in its character, and often attended with great prostration. In this chronic condition, the continuance of an astringent course, with a judicious use of tonics, becomes necessary. A close attention to the symptoms will be particularly necessary in this form, which partakes of the mixed character of a chronic diarrhœa, and may properly be denominated a sequel of cholera infantum.

This chronic condition will often continue without any very decided symptom of inflammatory action, at least to such a degree as to require direct anti-phlogistic remedies; an inflammatory irritation will exist, keeping up the morbid symptoms, and wasting the strength of the child by the constant discharge from the bowels. Under these circumstances, it will be necessary to resort to astringents, cautiously used, or to some of the stimulants which possess astringent properties, and which appear to exert some peculiar power on the mucous membrane. Nitric acid is a medicine of this kind, and in tedious cases will often act in a favorable manner by allaying the morbid action of the mucous membrane. Six drops of this acid, with six drops of the tincture of opium, may be diluted with an ounce and a half of water, of which a teaspoonful may be given once in three or four hours. When there is excessive relaxation an infusion of simarouba bark may be substituted for the water, and the mixture rendered slightly stimulating by the addition of the syrup of ginger or of oil of cinnamon, or a drop of the oil of cloves, rubbed up with sugar, and added to the mixture. In similar cases of atony, the sulphate of iron has been found useful in doses of one quarter of a grain three or four times a day. The tartrate of iron is also useful for the same object, that of imparting tone and astringing the parts. Ten grains may be dissolved in half an ounce of water, with the addition of one of the essential oils just mentioned; of this mixture, twenty drops may be given four times a day.

Throughout the whole of the chronic or secondary disease, debility is a prominent symptom; and while the discharges are restrained by appropriate remedies, whatever can impart tone by a proper stimulating influence should from time to time be used. One of the best we know of, is the compound tincture of cinchona, known as Huxham's tincture; the combination of the stimulating ingredients makes it peculiarly adapted to the restoration of the lost tone of the stomach and intestines. Tincture of kino, tannic acid, and other astringents may also be resorted to, with the addition of one of the

ssential oils, as some stimulant will be, in most instances, re-
uired in the greatly relaxed and prostrated condition of the
atient; the astringents scarcely acting of themselves. When
lcoholic stimulants are needed, we always prefer brandy to
ine, and always commence with brandy and water; if this
grees well with the child, we then combine the brandy with
inglass jelly, and if no untoward symptom arises from its
se, change it for milk punch, as supplying the most nourish-
ent. Often have we seen a child rally with surprising
romptness when a little brandy has been given. The symp-
ms are so varied and so combined, that a great variety of
edicines has been used to meet the different indications;
ost of them, however, of a tonic or stimulating nature, so
odified by suitable combinations as to meet the peculiar cir-
mstances as they arise. Other stimulants are of a specific
aracter, and act directly on the mucous membrane; of this
nd is the balsam of copaiba, which may be given in doses of
ree or four drops, twice a day. In this class, also, is nitrate
silver, already mentioned.

The instinctive desire for animal food at this time, is
gularly craving, and may safely be indulged. Butter, fat
ted meats, fresh meats abounding in juices, are eagerly
zed by the child, and are, perhaps, the best stimulants that
be given—far more efficacious than those of a therapeu-
nature.

In the management of this disease, our efforts must be
efly exerted in its incipiency, and during the stages when
till exhibits as its most marked phenomenon, the condition
ulting from an excessive and morbid development of the
ts which constitute and characterize the affection.

The transition from a healthy state in a young child to
t of disease, is rapid, both from high nervous sensibility,
from the quick interstitial growth of parts. The effects
he exciting causes show themselves promptly; and early
he disease are simple in their manifestation, decided in
r character, and with little or no complication. It there-
becomes a task promising more than ordinary results, to

examine well and thoroughly into the nature of the exciting agents, and their action on the economy of the system; to learn, if possible, in what manner the diseased development is connected with the ordinary development of the affected parts.

Regarding the process of growth as a great predisposing cause of disease among children, we have endeavored to trace the diseased condition in cholera infantum from its commencement as a simple natural development, and to connect this development with the well-known causes which are active in the production of the morbid changes in the parts affected in the disease, and have thereby been enabled to present the subject in a strictly practical aspect. The preventive and hygienic measures for relief, so abundantly proved by experience, have their full explanation in the philosophical consideration of the subject; while the same investigation bears directly to the establishment of the principles on which the therapeutic management is founded.

APPENDIX.

A.

It is remarkable that Dr. Cleghorn should for the first time have made the discovery referred to, in Minorca, and also that on his return to Great Britain he should not have recognized the disease of which he speaks, if it was a peculiar affection in his country. His talent for observation appears to have been unequaled. Dr. Fothergill, in a letter to Dr. Cuming, August 14th, 1742, awards the following praises to him: "Thou wilt, no doubt, admire the industry of our friend *Cleghorn*, who situated in a corner of the world, has made greater progress than any of us who even do not want the proper aids of study. Let us, therefore, stimulate one another, that we may follow his footsteps, and become the worthy friends of so great a man." —*Mem. of John Fothergill, M. D., by John Coakley Lettsom, p.* 104.

Dr. Cleghorn's observations in Minorca embraced a period of seven years. In three of these he mentions the existence of "cholera morbus" among children, in connection with several other diseases. In the other four years no mention of such affection occurs. The following is his only account:

1744. "From the end of June, the young children (who constantly suffer first by excessive heat and cold) are attacked with vomiting, purging, and periodical fever."

1747. "About the end of the month [June], the cholera morbus carried off many children."

1748. "The ensuing summer proved very unhealthy to children, many of them dying of cholera morbus."

It would appear from this that the disease was not uniform as to the time of its invasion; and the remark in the last mentioned year, seems to convey the idea that it was an unusual season for sickness among children.

The following extract is from the remarks of Dr. G. R. B. Horner, of

the U. S. Navy, on the diseases of Minorca, made from observations during the period between 1831 and 1838, inclusive: "Dysenteric and other disorders of the alimentary canal prevail, but I am not aware that the former does to such a degree as to be called epidemic; and in this respect the climate seems to have undergone a material change since Cleghorn lived in Minorca."

B.

The following table, taken from a paper by Drs. Niles and Russ, in the New York Medical and Physical Journal, v. 6, gives the number of deaths for eleven years of several prominent diseases, showing the influence of temperature on mortality in that city.

Diseases.	January,	February.	March.	April.	May.	June.	July.	August.	September.	October.	November.	December.	Total.
Consumption,	660	659	644	664	616	519	591	663	579	588	645	656	7466
Acute Diseases of Lungs,	200	292	254	250	210	114	117	105	92	141	152	172	2069
Fevers,	178	137	158	186	211	245	262	374	497	441	305	211	3205
Dropsies,	244	257	241	245	254	259	241	272	308	269	259	245	3044
Cholera Infantum,	2	2	2	1	5	32	246	527	288	108	14	18	1245
Dysentery,	22	14	29	7	16	28	233	489	378	203	79	36	1544
Croup,	137	106	122	106	91	66	69	67	84	140	139	122	1239
Tabes Mesenterica,	68	66	85	95	97	69	80	126	139	135	89	84	1115
Gastro-enteritis,	48	54	56	60	60	76	121	97	94	69	67	39	861
Hooping Cough,	53	66	42	33	37	31	73	105	105	71	56	49	721
Apoplexy,	49	59	56	47	57	45	109	59	45	59	55	57	697
Measles,	48	42	46	45	32	49	65	77	45	34	24	45	552
Intemperance,	49	32	33	31	49	47	46	43	44	47	63	47	531
Disease of Liver,	48	46	34	35	34	39	57	44	36	64	41	50	528
Palsy,	42	42	44	31	28	28	31	38	25	41	39	44	428

C.

The spring and summer of 1816 were remarkably cold. Various discussions appeared in the newspapers and philosophical periodicals, as to the cause. It was attributed to spots in the sun; to numerous ice-islands on the coast; and to the existence of a vapor which filled the atmosphere, "equally dense and yet diaphonous, all over the horizon, that existed at times in the atmosphere." This cold existed till the middle of June, with repeated frosts, which injured many wheat, rye, and corn fields in this and the neighboring States. In the latter part of June the temperature arose to 87°, but

again descended and continued to the 15th of July from 15° to 20° below summer temperature.*

In the year 1816, there was frost on Long Island in every month of the year, and the corn was killed almost universally by the 5th of September.†

Numerous reports were brought in by vessels arriving at New York, of immense ice-islands in the Atlantic, near the coast. Up to the 1st of July thirteen *different accounts* of these were published in the newspapers.

The temperature of the city of New York when compared with that of London is very remarkable. In the former city the mean of the summer is 75° : of the latter 60°. The highest range in London is 82°, in New York 97°. It is said by some that the disease exists in London; if so, it is certainly to a very limited extent, if, as we have seen, excessive heat is essential to its development.

D.

The dreadful pestilence that raged in Athens appears to have been of malarial origin, and to have affected all living creatures at a distance from human habitations. The sheep died in solitude. Lucretius observes, that even the birds in the woods, which they never deserted, and the beasts near the streams of water, and in fields and pastures, died in excessive numbers,—

> "Nec tamen omnino temere illis solibus ulla
> Conparebat avis, neque noxia secla ferarum
> Exibant sylvis: languebant pleraque morbo,
> Et moriebantur."‡

> "Hæc igitur subito clades nova, pestilitasque,
> Aut in aquas cadit, aut fruges presidit in ipsas,
> Aut alios hominum pastus, pecudumque obatus;
> Aut etiam subspensa manet vis aëre in ipso:
> Et, quom spirates mixtas hinc ducimus auras,
> Illa quoque in corpus pariter sobere necesse est.
> Consimili ratione venit bubus quoque sæpe
> Pestilitas; etiam pigris balantibis ægros."§

In the year of Rome 290 a severe pestilential disease spread throughout the city and country, affecting equally men and beasts. "Grave tempus et forte annus pestilens erat urbi, agrisque, nec hominibus magis, quam

* Med. Repository, 3 vol. New Series.

† History of Long Island by Henry F. Thompson.

‡ Lucretius, Lib. vi. 1217.

§ *Ibid*, Lib. vi. 1125.

pecori; et auxere vim morbi tenore populationis, pecoribus agrestibusque, in urbem acceptis."*

Lancisi mentions that in the Roman States the epizootics are very prevalent, and exhibit much the same symptoms as malarious diseases do in the man. McCulloch refers to their existence in Hungary, St. Domingo, Guadaloupe, France, and Italy, to a great extent, in 1711, 1745, 1772, 1785 and 1795, and in other countries, among all kinds of cattle, flocks of sheep, and wild beasts and birds,—all from paludal miasms.

Barron Larrey, in his surgical memoirs, speaks of the havoc made on the cows and oxen during Napoleon's campaign in Italy from the effects of malaria, thus: "Les reseignmens que je recueillie auprès des habitans, les recherches aux quelles je me livrai, m'assurèrent que l'épizootie reconnaissait, pour principales causes, la mauvaise qualité des fourages, l'état marécageux des pâturages, la chaleur excessive et prolongée qui avait succédée tout-a-coup à un printemps pluvieux et orageux. Les pluies d'orage avaient considérablement grossi les torrens, les rivières, et en avaient causé le debordement: une partie de ces eaux, aprés avoir nui aux récoltes, etait restée en stagnation dans les lieux bas et enfoncés; ce qui avoit formé autant de marécages."†

"Animals which feed in marshes where these fevers prevail, are found to have diseased livers. In the town of Wolcott, Seneca county, where marshes and low lands abound, the hogs, when killed, are generally found with corroded livers."‡

E.

The following remarks are from the report of the committee on the subject of the "Agency of the refrigeration produced by upward radiation of heat, as an exciting cause of disease." It also gives the amount of existing knowledge as to the immediate cause of the invasion of miasmatic diseases. "Most authors who have treated upon atmospheric influences to which persons are exposed, have confined their attention to the conditions of excessive moisture or malarious effluvia, the products of putrefaction. During the existence of such circumstances, according to our view, let any one be subjected to the chilling influences exerted through upward radiation from exposure during the night to the clear open sky, and fever of the specific character prevalent will be the result; or, in the absence of such epidemic

* Tit. Liv. lib. 3, c. 6.

† Mem de Chir. Militaire, tome 1, p. 165.

‡ Inaugural Dissertation on the Lake Fevers and other Diseases of the Genesee country in the State of New York. Edward G. Ludlow, 1823.

gents, diarrhœa, rheumatism, or other inflammatory affections may occur nstead."

"In cities there is a compensation for the loss of heat through radiation. The walls and pavements absorb more heat by day, than they can possibly hrow off at night. Hence the remaining heat prevents the detrimental effects of refrigeration from that partial radiation which alone can take place n cities. Persons can, therefore, go about at night in cities with almost s much impunity as by day, and this during the prevalence of epidemic nfluences which intrude themselves no farther than the precincts."

F.

Two long articles are in the Evening Post of September 20th and October st, 1828, on the subject of the origin and extreme virulence of the miasm. The Long Island Star of the 18th of September of the same year, has the ollowing editorial:

"*Malaria.*—The sickness which now prevails on Long Island, and which suddenly attacks whole families, is considered by some to be the malaria so well known in Europe, and which arises from heat, moisture, and he decay of vegetables. Its effect in some places has been dreadful indeed, nd we trust will call forth the investigation of gentlemen of medical talent."

The following are from the New York Enquirer of the same year: September 2d—"In several parts sickness has made its appearance in the shape f fevers, &c. Long Island particularly groans heavily, notwithstanding its roximity to the sea." October 3d—"It is very sickly on Staten Island, many families suffering for want of medical aid."

Journal of Commerce, October 3d, 1828:—"The epidemic fever, both ntermittent and remittent, which has been mentioned as prevailing in some arts of Long Island, still continues, and has increased of late, particularly n the vicinity of Newtown, Bushwick, &c. It is probable that it will continue unabated until we are favored with frost, that sovereign antidote to autumnal fevers. This city has enjoyed unusual health, and many families that moved into the country from fear of sickness precipitately returned. Our bills of mortality show that the number of deaths has fallen elow the average of years. It is a little remarkable, too, that all the ases of bilious fever of obstinate character which have occurred in New York, have been contracted in Long Island and New Jersey or neighboring country."

In that year, it is stated in the "*Statistical report on the sickness and mortality in the army of the United States*," that malarial diseases prevailed very extensively around Fort Hamilton, L. I. "The laborers employed in 1828 in the erection of this post—a locality which had been previously exempt from the effects of malaria—suffered greatly from intermittent and remittent fevers. The elevated coasts of Long Island in the vicinity of the Narrows, where a case of intermittent fever was unknown in the memory of the oldest inhabitant, became so rife with intermittent and remittent fevers as to drive the inhabitants from their possessions.

G.

By this table, made from the City Inspector's report, it will be seen what little influence was exerted on the mortality by cholera infantum by malarious epidemic of 1828, in the vicinity of New York. No remarkable sickness existed in any of the other years.

YEAR.	WHOLE NO. OF DEATHS.	DEATHS FROM CHOLERA INFANTUM.	
1825	5,018	151	1 in 33
1826	4,973	222	" 22
1827	5,118	238	" 22
1828	5,181	161	" 31
1829	5,094	119	" 42
1830	5,537	168	" 32
1850	16,978	713	" 23
1851	22,024	721	" 30
1852	21,601	915	" 23
1853	22,702	922	" 24
1854	28,568	1,525	" 19

H.

Extract from the report of the committee on the epidemics of Tennessee and Kentucky.

"*Cholera Infantum.*—This is a disease which has almost disappeared from considerable portions of this district, whilst it continues to prevail in other portions as much as ever. Notwithstanding much attention has been bestowed upon this disease in the United States, and especially in the west and southwest, it is doubtful whether the diagnosis is not even yet involved in some obscurity; *whether in fact, other diseases are not frequently called by that name.*"

"Cholera infantum usually begins early in the season, in May, for example, and frequently continues until October."*

In the table already given in this Appendix, the entire number for eleven years reported for the month of May, is five, out of twelve hundred and forty-five for the whole period.

I.

Extract from Emerson's Medical Statistics of Philadelphia:—"Bowel complaints, as the most destructive, stand first in this order (the mortality among children); of these about two-thirds are under the general designation of *Cholera*, nearly all of which were doubtless entitled to the specific appellation of *cholera infantum*, a disease almost peculiar to the United States, in many parts of which it exists endemically under the name of *summer* complaint. Of three thousand eight hundred and twelve, the whole amount of deaths reported from cholera, adults and children inclusive, three thousand six hundred and thirty-nine were under the age of puberty, and three thousand five hundred and seventy-six under the fifth year, viz.: two thousand one hundred and twenty-two under the first year, one thousand one hundred and eighty-six between the first and second years, *and only two hundred and sixty-eight* over the second year. As this affection seldom attacks them beyond the fifth year; the balance of two hundred and thirty-six, between the amount of that period and the total of all ages, may be counted as *cholera morbus*.†

J.

The following, from "Essays Medical and Moral," etc. v. 2, by Thomas Beddoes, M. D., will sufficiently prove the fact that heat alone will produce affections of the liver: "I was informed by an officer from the spot, that nearly all the men belonging to a regiment of horse in India, died within a short space of time of a liver complaint, because the commanding officer would persevere in exercising them in the face of the sun." In the appendix to the same work are found remarks by M. Fitzgerald, Esq.: "A gentleman well known to have practiced long and very extensively at Madras." He speaks, "from much experience of the facts," and states that great mortality occurred among Europeans by reason of diseases of the liver from heat, especially among a detachment which joined Lord Cornwallis' army in 1790, which

* Transactions of the American Medical Association, vol. vi. p. 333.

† American Journal of Medical Science, vol. i. 1827.

lost most of its men from this cause. He adds, that although intemperance may be a cause, yet "The natives, Dr. Beddoes may rely, sometimes suffer in very hot seasons, from acute inflammation of the liver without the assistance of intemperance, to which they are very rarely addicted." A very large accumulation of facts to the same point could easily be made, but these are deemed sufficient.

K.

DIET OF INFANTS.

Extract from an article by the Author, published in the "New York Journal of Medical Science,"—July, 1844.

The period of infancy presents to our attention circumstances in connection with diet, which have no parallel at any other time of life. In childhood and infancy the desire for food is frequent; capillary action, secretion, deposition, and interstitial increase, proceed with great vigor,—all, in consequence of this period being the period of growth; wherein it differs very essentially from the adult state,—as, in the latter, the action of vitality is comprised in the preserving and recuperative processes; to which is added, in the former, that of the development of the body. Growth is much greater during the first year; and the constant desire for food during that period is a circumstance of common observation. In young infants, the whole time is occupied with receiving nourishment and repose.

With this peculiarity of early infancy, there is also another more worthy of note, as it has a direct practical bearing on the subject before us; and that is in the fact that the food of all animals, in their earliest period of existence, is derived from animal substances,—even in those which afterwards subsist upon vegetable grains; the chick, even after being hatched, uses a part of the egg for its nourishment. Man, after infancy, obtains his nourishment from both animal and vegetable substances; and the great variety of articles of food makes a curious exhibition of his omnivorous nature,—the effect of custom, climate, religion, etc. The carnivorous propensities of the people at the extreme North, with their feasts of whale blubber, and the Indian Brahmin, with his meal of rice; the cannibal of the South Pacific Ocean, and luxurious tables prepared by the most scientific French cuisinier for the civilized convivialist; the oak-bark bread of one nation, the flesh-feast furnished by dogs, rats, mice, of another;—or the snakes, lizards, caterpillars, locusts, worms, and other reptiles, of the

African negro; and even the meal of clay used by the Otomacs, as mentioned by Humboldt, certainly present a wide variety in the articles used for sustenance, without considering the minutiæ of our daily food, to prove that man is omnivorous. But under all the circumstances of climate, whether in the frozen region of the North, or under the burning sun of the tropics,—of superstitious customs, or savage barbarity,—whatever be the circumstances influencing the character and quality of man's diet, there is for him in a state of infancy, but one kind of food—and that of an animal nature. Milk is his sole article of sustenance until the time when a change in his system takes place,—when a new arrangement of his organization requires him to seek his food among more varied substances, and to which his instinct invariably directs him. Comparative anatomy affords its evidence to the necessity of adhering to this one kind of food. The form of the infant's stomach is much more conical than that of the adult, and resembles in this respect the stomach of carnivorous animals; and the facility with which an infant vomits, or rather regurgitates its food, is analogous to the ease with which the dog or cat vomits whenever any thing is swallowed, even in an inconvenient manner. In the horse, rabbit, or hare,—animals altogether graminivorous,—the stomach differs very essentially in its form, resembling much more the stomach of an adult; vomiting can scarcely be excited, even with the strongest emetics. The nature of the infant's food may very rationally be inferred from this analogy.

If, therefore, nature has provided but one kind of food for the infant, a sudden change to substances essentially different in their nature must be injurious to a greater or less extent; this is a natural and direct inference from the facts already stated. In adults, where every species of food is used, an entire change, and a restriction to one course of diet, are always productive of important effects upon the general system; and where a scarcity of food has compelled large numbers of men to alter their food, and suddenly to adopt entirely different substances from what they were accustomed to eat,—as was the case in some parts of France, in the year 1817, when a failure of the crops obliged the inhabitants to submit to great transition, and to feed upon whatever vegetables could be found,—extensive disease and serious permanent effects are the results.

The influence of an entire alteration of food, from its powerful effects upon the system, causes it to be regarded as a measure of great value in the treatment of many diseases; and the change to vegetable food is not unfrequently an important remedial measure. In the adult, such a change is but the restriction of him to a class of substances that is natural to him, and which he can easily assimilate; and therefore does no violence to his physical nature. In the infant, on the contrary, the adoption of a vegetable diet is actually contrary to the arrangement of his digestive organs, and to the provision which is naturally made for his daily sustenance; and in the

diseases of the stomach and bowels, must, from the extra demand made upon them to assimilate substances foreign to their nature, add to the existing derangement. Acidity, flatulency, gripings, diarrhœa, and other symptoms of indigestion, are of very common occurrence in young infants; and indigestion is the most ordinary affection at that early period, showing how easily their digestion can be impaired. A very obstinate disease will sometimes arise in infants about the time of weaning, and particularly if the change from the bland food furnished by the mother be sudden; hence, the care usually taken to accustom the infant to a different species of food, by commencing feeding it some time before this period, and thus gradually accustoming it to the necessary alteration of diet. If this precaution is necessary to prevent injury where nature indicates that a different kind of food is required, how much more care is needed when greater injury is likely to arise on a sudden change, at a period when there is no such natural indication, and which it is so common a practice to disregard upon the invasion of any affections of the bowels. Immediately upon the appearance of these, arrow-root, tapioca or sago formed into jelly, rice or barley-water, is given, from the supposed bland and soothing qualities of these various articles, which, it is supposed, render them peculiarly applicable to the inflamed mucous membrane of the parts affected. Such is undoubtedly the case in the majority of instances in childhood and at adult age, but in infancy I very much question whether this course is followed by any other than injurious effect. I have watched carefully the influence of these substances for some years past, and am satisfied that the additional labor the digestive organs have to perform, in their attempts to assimilate an unnatural food, greatly aggravates the disorder of the bowels; and if they should fail in digesting the entire quantity, the remainder must be a foreign substance to the bowels, and thereby become an additional source of irritation. It is well known that vegetable food in general requires for its digestion more time and more energetic action of the digestive organs than animal food; flatulency and acidity are more frequent upon its use. Under ordinary circumstances, feculent nourishment passes much more quickly through the intestines than gelatinous, albuminous, or fibrinous food. It very often swells and undergoes some degree of decomposition, giving rise to the extrication of gas either in the intestines or stomach; and even when perfectly digested, hunger returns very quickly, whenever the patient is kept exclusively upon it. Some kinds of food are much more difficult to digest than others, and although there are a variety of circumstances in the individual which influence the alteration of sustenance taken into the stomach, yet the digestibility of food is very often affected by circumstances relating to the food itself. Thus, fixed oils and fat are slow of digestion, as is known to all dyspeptics; so also vegetable substances exhibit this indigestibility in a greater or less degree, when in a crude or raw state, or in a farinaceous or

any other artificial condition. If these effects are produced in the adult, for the reasons already mentioned, it must be much more the case in the infant; and many an instance of the obstinate perseverance of disease of these parts may be referred to the injudicious system of feeding during its prevalence.

It is a rare event to have the secrets of the digestive process revealed, and subjected to the test of actual experiment during life or on a postmortem examination. The former has been exhibited to us in the case of an adult, by Dr. Beaumont; and the well-known facts there elucidated have been considered of the most important character. Of no less value are the facts exhibited by postmortem investigations of children, made at Paris a few years since, by M. Natalis Guillot, for the purpose of ascertaining the condition of the contents of the bowels of such as died under the use of the ordinary diet of the hospitals. It is the custom at these and similar institutions, whenever an infant is sick to withdraw it from the breast, and to substitute for the milk some farinaceous substance made fluid by boiling; arrow-root, gummed rice water, or a thickened preparation of rice, known by the name of "Crême de riz," and other preparations of a similar nature, forming the diet of the sick infant. In the reported cases of the foundling hospital, and those for the reception of sick children, prescriptions of this nature form a very important part of the treatment, as will be seen by referring to the different treatises of French authors on diseases of children. The mortality in the French hospitals is very great; and opportunities are continually afforded for examination, in such numbers as to establish to a certainty almost any fact which requires the proof of anatomical demonstration. The attention of M. Guillot being directed to the changes which the food given to children underwent, and to the excessive mortality among them, he instituted a series of investigations in a number of cases of death, with special reference to the state of the contents of the bowels. He was struck with the uniform similarity; a jelly-like substance being present in the bowels, and in some instances lining both the small and great intestines. This was subjected to the test of the tincture of iodine, which produced an intensely blue color, thus proving it to be starch.

Here we have a direct proof of the deficiency of the digesting power; the articles given for food had passed through the length of the intestines, and had undergone very little change, and could only act as foreign and irritating substances. The almost entire suspension of the digestive process might indeed have occurred upon the use of any description of food; but when it is considered that a sudden change is usually attended with injury even in the adult; that vegetable substances are, under ordinary circumstances, more difficult to digest than animal; that the natural food of the infant is entirely dissimilar in its nature to what was at once substituted in the instances above mentioned; we might very reasonably expect to find a sus-

pension of the digestive powers a very serious aggravation of disease, and the results already stated.

From all the facts here given, it appears to be the most rational course to pursue, to preserve as much uniformity in the diet as the nature of the case and attending circumstances will admit. If in health, when the digestion is unimpaired, as we have seen food of an animal nature is the proper kind for the infant; in disease there can scarcely be any alteration to food of an entirely different kind, without incurring some risk. It is not my intention to assert that no alteration whatever is to be adopted, and because but one kind of food is provided for the infant, that it would be hazardous to depart from it under any circumstances; such a course would be to discard all sound theory, to abandon all attempts at controlling disease, and if fully carried out would reach even to the administration of medicine itself. While we avail ourselves of the plain suggestions of science and the accumulated experience of ages, let us so adapt them to the peculiar condition of a class of individuals as not to do a positive injury by their injudicious and indiscriminate application. The aged, the robust, the toil-worn laborer, the inhabitant of the city or of the country, the delicate female and the tender infant, all demand some modification in the application of the same universally admitted principles of medicine. In the case of infants we can, without removing them entirely from the breast, avail ourselves of the mild and unirritative effects of substances of the same nature as that upon which the infant feeds. One principal reason given for the adoption of vegetable mucilage, such as infusion of flaxseed, arrow-root, etc., is that it is a soft and soothing application to the tender and inflamed mucous membrane. Now if the same object can be attained by any substance that possesses the same bland qualities, and is also of the same nature as the infant's food, there will be no necessity for resorting to substances possessing such highly undigestible qualities as those of a vegetable nature. It is not that we may alter the diet from the compound aliment, milk; but to select from some simple alimentary substances, which it is deemed advisable to employ as medicinal agents, that which, from its essential composition, will be found more nearly allied to the nourishment which nature so abundantly supplies, and thus do no violence to the physical constitution.

NOTE TO PAGE 300.

We have to add to the postmortem examination, two cases recorded by Dr. W. C. Roberts, in the N. Y. Med. Gaz., Nov., 1841, one by Dr. J. A. Swett, one by Dr. Gross reported by Dr. W. Parker in the same journal, in the paper of Dr. Roberts, and one furnished to the author in MS. by Dr. J. L. Vandervoort. In all these five cases the mucous follicles were diseased; in three, the liver was abnormal; in one, reported healthy; and in one, that it appeared in this condition.

ON

ATELECTASIS OF THE LUNGS;

WITH A CASE.

By S. CONANT FOSTER, M. D.

The terms atelectasis, apneumatosis, carnification, and, finally, collapse of the lung, have been successively applied to certain states of the pulmonary tissue, of which the essential condition is the absence of air from the vesicles, its place remaining unsupplied by any morbid deposit. Although evidently seen and observed by Laennec and other writers upon pulmonary pathology of his day, and even before that period, it was always under a misapprehension of its real nature. Only since the time when Dr. Edward Jörg, adopting the views of his father, the Professor of Midwifery at Leipsic, called the attention of the profession to this peculiar condition, has it assumed its proper place as a distinct lesion. It illustrates, moreover, the propensity which exists to follow great authorities, even in error, when we state that although nearly a quarter of a century has elapsed since Jörg's first publication, it is still but imperfectly known; and we meet with allusions to it in quite recent writers—careful observers, too—under names which proclaim that they still retain the old and exploded views of its pathology. The successive researches of many investigators have, however, at length dispelled the former illusions, and assigned its rightful degree of importance to this very common pathological change.

Having had my attention called recently to this subject,

by the occurrence of a case in which the abnormity existed to a remarkable extent, and coupled with circumstances differing in some respects from those of any I have seen recorded, I propose to narrate it; and, having stated the conclusions to which it has led me, to glance at the history of scientific discovery upon this point; and, finally, to offer a brief synopsis of what is now the received theory and present state of knowledge in regard to it.

Case.—I was called, on the 20th of December, 1855, to visit M. M., æt. fourteen months; the child of healthy parents, and herself always healthy from birth, with some slight exceptions hereafter to be alluded to. When first seen, she was lying on the mother's lap, with head thrown back, breathing deeply and not very rapidly, but holding her breath at the end of each inspiration.

The countenance was flushed, the skin hot and dry, and the pulse rapid; but there was only a very slight cough, scarcely any hoarseness, and no croupy breathing. The bowels were somewhat costive, and there was great tenderness over the entire abdomen. Previous to my visit, the mother, supposing the child to be threatened with croup, had administered several large doses of syr. ipecac., in all about ʒ vi., which had failed to induce emesis.

Squills was now tried, and the fauces were irritated, but without emetic effect. The attempt to produce vomiting was therefore abandoned. The bowels were freed by castor oil and enemata; emollient cataplasms were applied to the abdomen, and onion epithems to the chest and throat. The gums, which were swollen, were freely lanced.

These means served to relieve the febrile movement. In the course of forty-eight hours, the skin became cool, the pulse lost its frequency, and the abdomen its tenderness. The peculiar respiration, however, still continued, with a very slight cough at long intervals. The child cried a good deal, slept scarcely at all, and refused almost all nourishment.

The chest was now examined, as carefully as the age and

condition of the patient would permit, with the following result: The resonance, on percussion, was good everywhere, except a very slight amount of dullness over the middle lobe of the right lung, both anteriorly and posteriorly. The respiration, elsewhere normal, here assumed a somewhat bronchial character. A feeble and distant muco-crepitant râle was heard at the points of dullness, which occupied but a very limited extent of surface.

There was nothing decisive in these phenomena, and the diagnosis remained obscure. All that seemed indicated with any degree of certainty, was the existence of a trifling bronchitis of some of the lesser tubes. An expectant plan of treatment was accordingly pursued; and the child, though slowly, seemed steadily to improve up to the 27th of the month, a week after the date of my first visit. On this day a slight diarrhœa occurred, with greenish discharges; and swallowing was observed to be attended with pain. An examination of the throat revealed a small ulcer, apparently approaching cicatrization, on the right tonsil. The child began to lose strength, and was ordered wine whey and the juice of beef, which it got in small quantities, and only by its being forced down.

Dec. 28. Dr. Macfarlan, a relative, saw the patient with me. Her condition was now as follows: The intelligence was unimpaired, and no symptom pointed to the brain. The skin was cool and soft, the pulse regular and not much accelerated. The diarrhœa had nearly ceased *sponte sua*, and the abdominal tenderness had wholly disappeared two or three days before. A slight difficulty in deglutition was noticed, and attributed to the state of the tonsil. The chest was again examined with great care, and with the same result as before; Dr. Macfarlan feeling doubtful in regard to the dullness and râles, so indistinct were they. The cough, which only occurred six or seven times a day, and was loose, and the peculiar respiration described above, were all that remained to indicate disease of any important viscus, or to account for the prostration which was now rapidly coming on.

The tonsil was touched with a solution of nitrate of silver; and it was decided to continue the stimuli and nourishment in as large quantities as could be conveniently administered, and to add a small dose of quinine every four hours.

Dec. 29. Prostration and dyspnœa increased, with more difficulty of deglutition. The patient sank rapidly, and died at 5 P. M.

Autopsy, Dec. 31, at 1 P. M.—Externally, nothing remarkable. The corpse was plump and fleshy; chest perfectly symmetrical, and everywhere resonant on percussion. (Dr. Macfarlan remarked that if any difference existed, it was less resonant on the left side, anteriorly and superiorly, than on the right.)

On opening the thorax, the heart and left lung were found perfectly healthy throughout. The cavities of the heart were empty; the ventricles contracted, as if death had occurred during the systole. The right lung did not collapse like the left, upon opening that side of the chest, but left a space of half or three quarters of an inch between it and the ribs. The anterior surface of the upper lobe was united to the thoracic parietes by old and strong adhesions, forming a circumscribed cavity, which contained about an ounce and a half of turbid serum, with a few shreds of recent lymph. Between the lower lobe and the diaphragm a similar cavity existed, holding not more than half an ounce. A little lymph was found here also. The adhesions were so strong as to require the knife to separate them. The proper cavity of the pleura was perfectly healthy, and free from fluid or lymph; the membrane smooth and shining. The lung contained no air, excepting what was found in a group of dilated vesicles on the superior surface of the lower lobe, which, as seen through the pleura, resembled a small collection of fine miliary tubercles. Upon cutting through them and making pressure, they disappeared. Section of the lung showed its tissue everywhere smooth, not granulated nor friable, but firm, like muscular tissue, and of a reddish brown hue with a slightly bluish tinge. Pressure emitted a small quantity of thin, sanguineous

fluid. A few drops of muco-purulent matter exuded from the bronchial tubes of the middle lobe. The tubes throughout the lung were thought to be possibly somewhat enlarged, but no obstruction was observed in any large bronchus.

In the abdomen, the liver was found large, but healthy; the gall-bladder containing about half an ounce of pale, viscid bile, which did not readily pass into the duodenum on pressure. Time did not permit more than a cursory examination of the other abdominal organs; which, however, seemed to be perfectly healthy. The brain was not examined.

I have said that this child had been always healthy, with some slight exceptions. I referred, then, to the following circumstances, which were difficult of explanation during life, but which find their interpretation in the morbid appearances after death. During the first week after its birth (the labor having been rapid, and unattended with any peculiar circumstances), I had been struck with the same peculiarity in the respiration which I have described above, namely, a retention of the breath at the end of each inspiration, and had more than once had it stripped and examined its chest to ascertain the cause of it. But the auscultatory signs were all healthy, and I remarked to the mother that there was something about the child's breathing which I could not fathom. This expression was recalled to my mind by the mother after the child's death. At two months old it was attacked with acute eczema, which made a raw surface completely around the neck and over the ears. At this time the breathing was again noticed and the chest examined carefully, without result. The child soon recovered its usual state of health, and the interrupted respiration, though it continued, remained unaccounted for, but ceased to be a source of anxiety. It was the same thing, only more marked, during the illness of which it died. In connection with this symptom was the fact, that the child always slept on the right side. When placed on the left side, it would not cease to worry until turned; and if it ever lay upon the back, the head was thrown strongly back, as I have described it at my first visit during the last illness. The only

other deviation from perfect health, was a tendency to constipation for a few weeks before its death, with clay-colored evacuations, which had been relieved by gentle laxatives and cholagogues. These phenomena are now, in a measure, accounted for. I am convinced that the pleuritic adhesions were congenital, and were the cause of the peculiar decubitus and respiration. That they were not detected during life, will not be surprising to those who are familiar with the difficulties attendant upon a physical exploration of the chest in infancy. I believe them to be congenital, because the character of the respiration was such as would be very likely to proceed from such a cause, and not likely to proceed from any other; because this symptom had undergone no material change since the patient's birth; and because there was no other period of the child's life at which the lesions could be supposed to have occurred.

It remains to be determined how far these lesions exercised an influence in producing the extensive collapse of the lung, which resulted in death; and whether the recent inflammation of the shut sacs above described, was a cause or a consequence of the collapse, or a concurrent effect of some third cause.

These questions cannot be answered in a manner entirely satisfactory; but much reflection on the subject has led me to the following conclusions: 1st. There was no disease in this child's lungs prior to the ten or twelve days before its death, if we except the old adhesions of the lung to the pleura, which I assume to be congenital; 2d. A bronchitis occurred while the child was in a somewhat debilitated condition, owing to the irritation from teething. This affected chiefly the middle lobe of the right lung, and led to atelectasis of that lobe, and perhaps of lobules in other portions of the lung; 3d. The parts of the lung so affected, collapsing, increased the embarrassment of the respiration; and the hyperæmia (due to mechanical causes) thus produced, added to the traction now made upon the old adhesions, led to inflammation and effusion at those points; 4th. These circumstances, react-

ing, tended to increase the extent of the atelectasis, which finally involved nearly the whole lung.

This theory of the case is, I admit, imperfect. Supposing it to be correct as far as it goes, it fails to account for some of the physical signs, especially the resonance on percussion. If we could admit the possibility of the existence of a certain quantity of air in the pleural cavity, it would aid us materially. I do not feel warranted in adopting that hypothesis, though I cannot otherwise account for the phenomenon in question. In any event, the case must always remain an exceptional one, and would be interesting on this account, if on no other, that while it illustrates, on the one hand, the general correctness of diagnosis based upon physical signs, as has been seen with regard to the bronchitis found in the middle lobe, on the other it exhibits in a very striking manner certain possible sources of fallacy, which should teach us not to rely too exclusively upon them, even after careful observation, especially when we have to deal with the pulmonary affections of young children.

I pass now to the general subject; and in the commencement, I would state that I retain the term "atelectasis" to express all the varieties of this affection, for several reasons. In the first place, it was bestowed by the pathologist who first accurately pointed out its nature, and has been more generally adopted in scientific works than any other; secondly, it is sufficiently comprehensive to embrace all the forms of unaërated lung which spring from similar causes; and will serve to designate a different condition from the compressed lung of pleurisy, and the congested lung of certain fevers; and, lastly, it belongs to the language of science, and no objection lies against it excepting the difficulty of pronunciation, which is trivial. On the other hand, the term collapse (contended for by Dr. Gairdner, and which has much to recommend it) is not strictly applicable to the persistent fœtal condition; while *état fœtal*, splenization, and the rest, are of still more limited application, besides being objectionable on other accounts.

If we examine the different treatises upon pulmonary pathology and the diseases of infancy, prior to 1835, we shall find abundant reference to, and descriptions of, a peculiar form of pneumonia to which children were subject from their birth; and which differed essentially from the pneumonia of adult life. In the year referred to,* Jörg published his treatise, "Die Fötuslunge im gebornen Kinde;" in which he showed that, so far as concerned newly-born children, this affection was entirely distinct from pneumonia, and independent of all inflammatory action whatsoever; and that it was merely a persistence, in certain portions of the lung, of the unexpanded condition of the organ during fœtal life. He accordingly gave it the name of atelectasis, from the Greek words ἀτελὴς, imperfect, and εκταςις, expansion, or dilatation. The announcement, however, seems to have attracted but little attention, though it has since proved the key to a vast deal of important knowledge; for we still find eminent writers falling into the old error, and devising various theories to account for the peculiarities of an inflammation which was no inflammation at all. Thus we find, after it had been seen this condition existed not only in infants of the earliest age, but up to the fifth or sixth year, Rilliet and Barthez endeavoring to distinguish between the acute and chronic types; Billard speaking of it as essentially congestive, and supposing the arrested blood to act as a foreign body; Valleix and Bouchut, and Cruveilhier, and many German authors, continuing to deceive themselves, not as to the appearances of the affected organs, for they were unmistakable, but as to the nature of the affection. They speak of "broncho-pneumonia," "catarrhal pneumonia," and the "lobular pneumonia of infants," in contradistinction to pleuro-pneumonia, and the *lobar* pneumonia of adults.†

* He had, two years previously, expressed the same view in a Latin dissertation "*De Pulmonum Vitio Organico.*"

† One of the earliest to perceive that essential differences existed between the two affections, was our distinguished countryman, Dr. Gerhard. In a valuable paper published in the American Journal of Medical Science, in 1834, he

At length, however, MM. Bailly and Legendre, by a very simple experiment, furnished a test whereby all doubt was at once removed. This was the forcible inflation of the condensed portions. Their researches were published in the "Archives Générales de Médicine," in 1844. By the simple means they proposed, to use the language of Dr. West, "they have thrown more light on the affections of the lungs in infancy and childhood, than all the writers of the previous ten years taken together." But even after this decisive test had been proposed and applied, it was objected that, in certain cases of undoubted inflammation, it was possible to produce dilatation of the air vesicles, to a limited extent, by forcible insufflation. Here, however, the microscope, which has done so much for modern pathology, came to our aid, and proved conclusively the distinction between the two states; showing that, while in the one there was the usual inflammatory deposit, in the other the normal elements of the lung-tissue alone existed.

The truth is, contrary to the generally received opinion, that true pneumonia is a rare disease before the age of five years; and when it does occur it affects lobes and not lobules, is ordinarily confined to one lung, and in all other important respects resembles the same condition as it is observed in the adult. Indeed, so distinct is this affection from pneumonia, that it would seem that the portions of lung which are in the state of atelectasis, are less prone to inflammation than the surrounding portions, and even may oppose a barrier to its extension. "I have seen," says Hasse, "a case of genuine pneumonia with hepatization of the inferior lobe, one portion of which, being *atelectatic*, had not participated in the inflammation, but presented hard, knotty, depressed patches, in the midst of expanded, softened, hepatized substance."*

Dr. George A. Rees, of London, claims to have been the first

gives the result of his observations in the Children's Hospitals of Paris, and contrasts the lesions with each other very skillfully; still, however, without wholly divesting himself of the idea of inflammation.

* "Pathological Anatomy," Sydenham edition, p. 251.

after Jörg to call attention to the occurrence of atelectasis "as a disease after birth;" but, without detracting from his merits, it is certain that the second great step in the investigation was the insufflation test of Legendre and Bailly. Since the publication of their researches, progress has been more rapid, and the subject has been pursued by several writers, among whom may be mentioned Hasse, West, and especially Dr. Gairdner, of Edinburgh. In the writings of the last-named author will be found an elaborate critical analysis of the opinions of those who had preceded him, and an able statement of the probable causes, symptoms, and effects of this pathological change, as it occurs not only in childhood, but in adult life. Dr. Gairdner has done more than any one with whom I am familiar, towards a complete elucidation of the subject, by tracing the connection between atelectasis and other pulmonary affections at all periods of existence.*

With this cursory glance at the course of modern discovery in this branch of pulmonary pathology, I proceed to state more clearly than has yet been done in this paper, in what the pathological condition consists, and, by a consideration of its causes, symptoms, and effects, to show how important a part it plays among the diseases of the respiratory system. And first of the atelectasis of the newly born. When a child first breathes upon coming into the world, if it is vigorous, the whole of its lungs are expanded and filled with air. But debility, or the impaction of mucus in the bronchial tubes, may prevent this; and then if the vital powers are not sufficient to overcome the difficulty, the child dies in a few days, and the following state of things is the result. Scattered through one or both lungs, generally both, will be found isolated lobules which have received no air, but remain precisely in the condition of fœtal life. These lobules are of a bluish-red tint, are somewhat depressed below the level of the surrounding lung,

* See his work "On the Pathological Anatomy of Bronchitis," &c. Edinburgh, 1850. Also Brit. and For. Medico-Chirurgical Review for April, 1853.

do not crepitate upon pressure, and are of a firm and tough texture. The cellular interspaces between the lobules are distinctly marked. Portions separated from surrounding lung, sink in water. On cutting into these portions, the knife leaves a smooth surface, shining, not granular nor friable as in hepatized lung, but closely resembling muscular tissue, and either entirely dry, which happens when the portion affected is small, or yielding to pressure a little thin, bloody fluid. The parts most distant from the roots of the lungs exhibit these appearances to the greatest extent, namely, the lower and posterior portions, and the thin edge of the upper lobes. Occasionally an entire lobe is involved, in which case it is generally the middle lobe of the right lung. Very rarely indeed the whole of one lung is affected. Within a few days after birth the lungs admit of complete inflation. The subjects of this affection are, almost without exception, weakly children; and abroad it is often found associated with that indurated condition of the external cellular tissue rarely seen here, but of frequent occurrence in the foundling hospitals of Europe. The foramen ovale is commonly found more or less open.

The variety of Atelectasis which occurs after birth, where portions of the lung which have once received air fall back into the fœtal state, occurs as a sequel of acute or chronic bronchial catarrh, and depends in most instances upon the stoppage of certain of the bronchial tubes by mucus or pus. The altered portions present almost precisely the same appearances as in the first variety, except that their darker color more readily strikes the eye in contrast with the surrounding healthy tissue. As in the former case, where it has not existed long, the parts can be perfectly inflated and restored to the normal condition. Both varieties differ from pneumonia in this important respect, as well as in firmness of tissue, diminished bulk, absence of granulations, and absence of inflammation of the pleura.

Neither variety necessarily destroys life; but the collapsed portions may remain, in which case they shrink more and

more, and become constantly denser; and there is reason to believe that certain of the cicatrices so often found in otherwise sound lungs are due to this cause.* Some pathologists, among whom is Hasse, suppose that the same may be said in regard to the small calcareous masses sometimes seen, especially in the posterior portion of the lung.† Where extensive atelectasis is recovered from, it is probable that more or less deformity of the chest is the result. The lower ribs are drawn in laterally, and the peculiar conformation known as the "pigeon breast" is produced.

But perhaps the most important of all the consequences of atelectasis, considered in relation to other diseases, is the production of emphysema. It is obvious that aside from the contraction of the thoracic parietes, which must of course be a gradual process, nature has to contrive some mode of compensation for the diminished bulk of collapsed lung. At each inspiration a vacuum is created, to supply which we have two things, viz., air and blood. Hence, in atelectasis of any considerable extent or duration, we are certain to find more or less of hyperæmia and of emphysema. It would transcend the limits I have assigned to myself in this paper, were I to attempt to trace out minutely the mechanism by which these conditions are produced, and the remote effects which ensue from them in the circulating and respiratory systems. Let it suffice for the present, therefore, to state the facts—which are believed to be susceptible of demonstration, —that the congestion which the older pathologists regarded as a cause, is rather a consequence of atelectasis; and that many cases of emphysema, hitherto of obscure origin, are dependent upon atelectasis as the proximate cause.

From what has already been said concerning the conditions under which atelectasis is found, it will be seen that the causes of this condition readily arrange themselves under two

* Fœrster says that they undergo a species of fatty degeneration. See "Handbuch der Speciellen Pathologischen Anatomie." Leipsic, 1854, p. 157.

† Hasse. "Pathological Anatomy." Sydenham edit. p. 250.

heads, viz., whatever occasions imperfect action of the muscles of respiration, and whatever impedes the passage of air through the bronchial tubes. On the one hand, general debility, existing at birth, coming on soon after birth from prolonged exposure to cold, &c., or ensuing from protracted and exhausting diseases, as chronic diarrhœa and dysentery; and on the other hand, bronchitis and bronchial catarrh, foreign bodies in the air passages, &c. These are causes which may operate at all periods of life; and accordingly we find that no age is exempt from atelectasis, but that its most perfect types are found at the two extremes of existence, when the vital powers are feeblest and the voluntary efforts least adequate to remove any obstacles which may exist to the performance of the function of respiration. Atelectasis is found accompanying the suffocative catarrh of the aged, identical in every important respect with that of the new-born babe.*

I cannot leave this part of the subject without alluding to the relations which exist between atelectasis and two of the most important diseases of childhood. I mean measles and hooping-cough. In both of these disorders we have been taught to regard the principal danger as arising from inflammation of the lung. In regard to the former, statistics are wanting; but as to the latter, we have data sufficient to warrant the assertion that atelectasis, and not pneumonia is the lesion which causes death. In a very recent work published in London by Dr. Graily Hewett, he gives the results of an epidemic of hooping-cough in a public institution under his charge. Nineteen autopsies were made. In all but one, which was extensively tuberculated, this lesion was found. In every instance, both lungs were affected. Both lower lobes in all of the eighteen cases, the right middle lobe in sixteen, the left upper in fifteen, the right upper in ten (four of

* Mendelssohn and Traube produced atelectasis upon animals, by the insertion of plugs of different sorts into the bronchi. The animals so treated died within a day or two, when the portions of lung supplied by the plugged bronchi were found in the state of atelectasis. Similar experiments have recently been made in this city by Dr. Von Roth, and with like results.

these but slightly). The collapsed portions were congested in seven cases. The air-cells adjoining the collapsed portions were slightly, but visibly, enlarged. Pneumonia was found but in four cases, and in only one to any extent.

This account of Dr. Hewett, for which he deserves the thanks of the profession, finds singular confirmation in a paper published some twenty-five years before. In a brief essay by Dr. Alderson, to be found in Vol. XVI. of the Medico-Chirurgical Transactions, he gives the details of four cases, in which the same lesions are described with remarkable accuracy, and says that in many others which he had inspected, he had invariably found the same appearances. He insists that these appearances are very "different from those found in peripneumony," and controverts Lænnec's views concerning hepatization. He adverts to the dilatation of the air-cells and lesser tubes, and sees in them "a foundation for future asthma." It seems quite extraordinary that this remarkable paper, in which the author had so nearly arrived at a correct theory of atelectasis, should have been so long overlooked.

The symptoms of atelectasis need occupy us but a moment. Dr. G. A. Rees thinks that a pathognomonic sign exists in the altered movement of the ribs during respiration. He describes this as follows: "During the inspiratory effort [the diaphragm being in early life the chief muscle of respiration], the ribs move inwards, or backwards towards the mesial line of the trunk, instead of outwards as in ordinary respiration, diminishing instead of increasing the transverse diameter of the thorax." It is in this way he "supposes" that the "pigeon breast" occurs. There is cough (except in the congenital cases, when laryngismus seems to take its place), generally paroxysmal, but proportioned in severity to the extent of the bronchitis; and more or less dyspnœa, which however is seldom distressing. A certain amount of fever attends the commencement in cases occurring after birth, but soon subsides. Diarrhœa is of frequent occurrence. Beside these there is no other rational sign, excepting great prostration. Towards

the close, the patients are not unfrequently convulsed. The physical phenomena are somewhat uncertain, owing to the two lungs being for the most part nearly equally affected, which deprives us of the advantages of comparison. With the aid of the rational signs, however, we shall generally arrive at a correct diagnosis. There is ordinarily more or less dullness on percussion; though, in rare cases, as we have seen, the entire chest may be perfectly resonant. The complete flatness of pneumonia is never present. The respiration remains nearly normal, but feeble, and rarely assumes a bronchial character. The râles are generally few, and of the mucous or muco-crepitant character.

As the chief object of all pathological investigations is to improve our means of treating disease, I shall close this paper with a few remarks upon the treatment of atelectasis. And here the first consideration which suggests itself is, that it is, in all its varieties, essentially a disease of debility. That, excepting the bronchitis which exists in some cases, it is in no manner dependent on or likely to lead to inflammation. We must entirely disabuse ourselves of the dread of pneumonia; which is, in truth, as I have before said, an exceedingly rare affection under the age of five or six years, and which, when it does occur, is almost as easily distinguished as in the adult. But knowledge in regard to the therapeutics of this affection has always, or at least of late years, been in advance of knowledge of its pathology. The danger is not great, at the present day, of a child with a feeble pulse and difficult respiration, and a cool skin, being subjected to a treatment calculated to reduce still further the powers of life, even though cough and dullness on percussion should lead the physician to suspect pneumonia; and bronchitis, even of considerable intensity, is ordinarily understood not to require or to bear much antiphlogistic treatment. Yet the fear of aggravating an already existing inflammation of the pulmonary parenchyma, might even now deter many from adopting the means which are absolutely demanded in order to save life. Moreover, it is not so very long ago since the treatment em-

ployed was of such a nature as, it is to be hoped, no educated physician of the present day would have recourse to. It will be instructive to refer once more to the paper of Dr. Alderson, which I have already quoted, to see what sort of treatment *he* could adopt, who had so nearly emancipated himself from the errors, at least in regard to the pathology of hooping-cough, entertained by his predecessors. The four cases which he recounts were aged respectively seven and a half, and nine months, two, and three a half years. One (two years old) was "freely leeched and blistered, and took saline mixture, with digitalis and conium, with warm bath to the lower extremities; she took no other food than barley water." Died in a day or two. The next (this was the youngest subject, seven and a half months old), had "three leeches applied to the chest, a small blister for four hours between the shoulders; a grain of calomel, and an eighth of a grain of tartarized antimony every eight hours." On the second day after this we find him described as "very sick from the antimony," &c., &c. Next day "system affected by mercury," and so on. "Died in the afternoon." The third (nine months) had "an ounce of blood taken from the chest by cupping," and afterward "four leeches were applied to the temples;" "small doses of calomel and antimony every three hours;" and the next day, "medicines continued." There was no occasion to continue them the following day. "Died at 9." The fourth and last (three and a half years old), had "six leeches to the throat," "a smart purge of rhubarb and calomel;" "in the evening, a fluid drachm of ipecac. wine every ten minutes till she vomited." Next day "calomel and antimony, with saline mixture and nitric ether;" and the next—death! And after all this the author complains that leeches do not take blood fast enough, and recommends the "prompt and bold extraction of blood by cupping," &c., &c.

This was the result to which false views of pathology led a very intelligent and careful observer. I am loth to believe that such treatment as this could be pursued in our day. The very opposite course is required. A stimulating plan

ould be pursued, generally, from the commencement of the se. In the congenital variety, the greatest care should be ken to keep the child warm. The hot bath should be adinistered once or twice a day, and in threatening cases, ustard should be added to it. The muscles of the thorax d abdomen may likewise be stimulated to action by fricn and the use of liniments; and brandy, ammonia, or other ffusible stimuli administered internally. Nourishment by eding, if the child is unable to suck, must not, of course, overlooked; and the bowels should be regulated by the ildest means. The same plan is equally adapted to the acired variety, waiting only in certain cases until any febrile tion, which may spring from bronchitis, shall have subsided. fter this, stimulation, both external and internal, can be sorted to very freely; and we should not be deterred from rsuing this course, I hesitate not to say, even by the occurnce of convulsions (which, in these cases do not of themlves prove the existence of any inflammation within the anium). Only in the presence of some well-marked phlegmia, should the stimulating course be modified. An occaonal emetic will be useful by serving to dislodge the mucus om the bronchial tubes. The best for this purpose is squills. ernutatories would perhaps prove useful, and electricity ems worthy of a trial. I make the suggestion without havg tried either.

If by these means we do not save our patients, we shall at ast have the satisfaction of knowing that we have not hastned their death by treating antiphlogistically maladies hich have nothing in common with inflammation.

REPORT

ON

ALGA CHOCOLATE AND BISCUITS.

By the Section on Public Health and Legal Medicine.

READ DECEMBER 3, 1856.

The Section on Public Health and Legal Medicine, to which was referred the communication of Messrs. J. & I. Coddington, of this city, respecting certain articles of diet alleged to contain Iodine, derived from combinations of ordinary edibles with certain varieties of Marine Algæ, together with specimens thereof, Report as follows :—

Few persons are ignorant of the fact, that many varieties of the Marine Algæ, or Sea-Weeds, possess considerable value as articles of nutriment, not only for the inferior animals, but also for man. In Great Britain, they are beginning to be extensively employed as food for young stock, such as pigs and calves ; and every one knows that, in seasons of famine in Ireland, they have been successfully resorted to by the indigent peasantry on the coasts, as the means of supporting life. Used, however, for the latter purpose in their natural state, and without suitable preparation, they have not been found sufficiently palatable and digestible to be brought into general use as food, except as an extreme resort.

Some few of the varieties, such as the chondrus crispus

(carrageen, or Irish moss), which abounds on all the shores of Europe, and on the northeastern coast of North America, possessing much soluble matter which has the property of gelatinizing great quantities of milk, have been somewhat extensively used, abroad and in this country, in the preparation of *lanc-mange*, and similar articles for the table; but rather as matters of luxury, than with any reference to their nutritious or dietetic value.

Other varieties, such as the rhodomenia palmata (*dylisk or dulse*), are, in their natural state, freely eaten upon the coasts of Ireland, and seems to be agreeable to many palates. Some of these are collected in considerable quantities on the coasts of Nova Scotia and New Brunswick, and we not unfrequently meet them on sale in the streets of this city, where they are recommended by their venders as promoters of appetite and digestion, and also as vermifuges. The dulse has, however, been long used in the Mediterranean as a valuable ingredient of soups, and the ill success which attended the praiseworthy efforts of the far-famed cook, Soyer, to employ it in the same way in the last great famine in Ireland, affords a melancholy illustration of the influence of poverty and ignorance, in blinding their victims to their own best interests.

Several varieties of the algæ, including the one just named, abound in that form of sugar known as *mannite* by chemists; although we have no direct experiments upon the subject, it is, we think, fairly presumable, from its constitution and its general chemical habitudes, that its action upon the animal economy is similar to that of ordinary sugars. One of these algæ, the laminaria saccharina, which yields, according to Stenhouse's analysis, no less than 12.15 per cent. of mannite sugar, is found in abundance on rocky shores, from Greenland to New York, at four or five feet below low water mark, and is cast up from greater depths on the shores of New Jersey.

All the varieties of marine algæ, which have been examined, have been found to contain *iodine*, existing in combination with potassium, sodium, or magnesium. This elementary

substance, so interesting in its chemical history, and important in its medical and technical applications, was first discovered in them; and thus far, they are the only sources from which it has been procured. They abound also in the phosphate of lime, that most important material for forming the more solid portions of the structure of the higher classes of animals,—in the fixed alkalies, of, perhaps, equal importance; and above all, in those nitrogenous, or *protein* compounds which are universally recognized by physiologists as the special, immediate sources of nutrition to animal life.

These sea-weeds, in fact, perform in the vegetable kingdom of the sea, a function of the same character with that of land vegetables in *their* sphere. They appropriate and condense within their tissues, materials sparsely distributed in the inorganic world—bring them into the constitution of organized matter, and fit them for the support of animal life. There can, indeed, be no doubt that they constitute, directly or indirectly, nearly the whole of the food of all the animals that inhabit the ocean.

These considerations, based principally upon the information contained in the admirable contributions of Dr. Harvey, to the publications of the Smithsonian Institution, on the marine algæ of the North American coast; and in the opening article of the *Edinburgh New Philosophical Journal*, for July 1, 1856, *On Certain Sea-weeds of an Edible Kind, by Dr. John Davy*, seemed to Messrs. Coddington of sufficient importance to justify an attempt to bring into practical use the principles that are fairly deducible from them.

We do not know that we can better explain their object, than by quoting largely from the last named article.

Dr. Davy, after describing the general characters of four or five of the best known of the edible algæ, and determining the properties of the substances yielded by them to the action of solvents, concludes his remarks in the following way:—

"That these sea-weeds are deserving of the reputation "which they have among the poor, to whom their use as

"articles of food, with one or two exceptions, is chiefly re-
"stricted, and most of all in Ireland, is, I cannot but think,
"in a manner demonstrated by the experiment of the able
"chemist whom I have just named, (*Prof. Apjohn, of Trinity*
" *College, Dublin,*) and on whose accuracy the most perfect
"reliance may be placed. I was present when some of
"the first trials were made in the laboratory of Trinity
"College, for the purpose of ascertaining the proportion of
"nitrogen by burning them with soda lime; and I can bear
"witness to the care with which the process was conducted.
"Dr. Apjohn, in an interesting lecture which he delivered
"last spring, at a meeting of the Royal Agricultural Society
"of Ireland, and since published in the Society's Journal,
"has given some account of these experiments, and he has
"favored me with the results, with some additions, with per-
"mission to use them. I shall give them in a tabulated
"form, as drawn up by him, with a statement for compari-
"son also by him of the proportion of nitrogen in some of
"the vegetable substances in most common use as articles
"of food for man and cattle."

EXPERIMENTS MADE BY DR. APJOHN IN CONJUNCTION WITH DR. DAVY, IN JULY, 1854.

		Nitrogen, per cent
Specimen supplied by Dr. Davy, and dried at 212°	*Chondrus crispus*, bleached,	2·152
	Fucus vesiculosus,	2·379
	Rhodomenia palmata (dylisk)	3·376

EXPERIMENTS MADE BY DR. APJOHN IN AUGUST, 1854.

SPECIMENS.	Water.	Dry Matter.	Percentage of Nitrogen in Dry Matter.	Protein contained in Dry Matter.
Chondrus crispus, bleached, ...	17·92	82·08	1·534	9·587
" " unbleached, . . .	21·47	78·53	2·142	13·387
Gigartina mammilosa, ...	21·55	78·45	2·198	13·737
Chondrus crispus, bleached, 2nd exp. .	19·79	80·21	1·485	9·281
" " unbleached, 2nd exp.	19·96	80·04	2·510	15·687
Laminaria digitata, or dulse tangle .	21·38	78·62	1·588	9·925
" " " black tangle .	31·05	68·95	1·396	8·725
Rhodomenia palmata, or dylisk . .	16·56	83·44	3·465	21·656
Porphyra laciniata, or levre	17·41	82·59	4·650	29·062
Iridæa edulis	19·61	80·39	3·088	19·300
Alaria esculenta, or murlins	17·91	82·09	2·424	15·150
Means	20·42	79·58	2·407	15·045

Percentage of nitrogen in various edible substances, dried at 212°.

	J. A.
Potatoes	0·541
Flour, of first quality	1·817
Beet roots, mean of 13 experiments	1·848
Mangolds, mean of 13 experiments	1·781
Swedish Turnips, 5 experiments	1·843
Mean	1·567

"These results are so unexpected, that, could there be "any doubt, which I have not, of their accuracy, it might "be questioned. The mean of them shows that the propor- "tion of nitrogen these algæ contain exceeds that, not only "of the ordinary articles of vegetable food, but even of "wheaten flour, being as 2·407 to 1·817.

"Are not, I would ask, these esculent sea-weeds, on account "of the iodine and bromine which they more or less contain, "deserving of more general use? Nowhere, I believe, where "they are in common use, is bronchocele known; and, as far "as I have been able to ascertain, scrofulous complaints are "rare, and even pulmonary consumption;—while on the "contrary, bronchocele and scrofula are more or less pre- "valent, especially among the laboring class, who rarely "have the benefit of articles of food known to contain "iodine,—and this remark applies to our troops, whose "dietary has hitherto been regulated with such a marked "neglect of medical science. In the West Indies, phthisis "was unusually prevalent while I was there, in the eighty- "eighth regiment, the Connaught Rangers, a fine body of men "raised chiefly in the province, the name of which it bears, "men, who at home, we are tolerably certain, made use of "sea-weed occasionally and sea-fish, while in foreign service "their diet was almost exclusively bread and meat, with a "scanty addition of vegetables.

"In relation to use, these vegetables have most of them "the recommendation of cheapness, so as to be within the "reach of the poorest; and their not being liable to spoil by "keeping when dried, is another argument in their favor; "and another, I may add, is that they are grateful to the "palate, and some of them—dulse, for instance, even in "their undressed state. The *blanc-mange* that is made by

"substituting chondrus crispus for isinglass, is hardly distin-
"guishable from that prepared with the more costly article,
"and is in common use in Ireland at the tables of the
"opulent.

"Everywhere, where sea-weeds are easily obtained, they are
"in repute as manure. Their fertilizing powers are quite in
"accordance with their composition, the large proportion of
"azote, and the considerable portion of the inorganic sub-
"stances they contain, and which are equally the elements
"of our cultivated crops, especially phosphate and carbonate
"of lime, and one or both of the fixed alkalies.

"The part which these marine plants perform in the econo-
"my of nature we are sure is considerable, and, probably,
"is little less important than that exercised by terrestrial
"plants. May they not be considered as the purifiers of sea-
"water, tending always to remove the excess of carbonic
"acid and probably of azote? And may they not be viewed
"as the restorers (they certainly are the collectors) of sub-
"stances possessed of excellent medicinal qualities, viz.,
"iodine and bromine, which so specially belong to them,
"and in them seems providentially saved and stored up for
"the use of man?"

Such being the character and virtue of the algæ, as established by the most unimpeachable scientific testimony, the idea was conceived by Messrs. Coddington, that probably a great service might be rendered to a very large class of valetudinarians if they could be supplied with a diet that should combine the nutritious and medicinal virtues of these algæ, especially of those most abounding in iodine, with other common articles of food, to form compounds that should be palatable and digestible. Some of these they have already successfully prepared, and experiments are in progress upon a variety of others, which when complete, they intend to present to the public.

The preparations to which they have called our attention, are two varieties of alga bread or biscuit, and an alga chocolate.

Alga biscuit, No. 1, is described as follows :—

This biscuit is made from the *fucus vesiculosus*, one of the algæ most abundant on our coasts, in combination with wheaten flour. The alga, after being washed and dried, is ground into flour and thoroughly incorporated with the wheat—then made into biscuit. The odor and flavor of this biscuit immediately recalls those of shell-fish, and are found almost uniformly agreeable to the palate. They are eminently digestible and nutritious; and abounding in iodine, as does the alga which they contain, may be reasonably expected to suit those forms of disease for which that remedy is indicated, such as scrofula, phthisis pulmonalis, bronchocele, etc.

Alga biscuit, No. 2, is thus described :—

This biscuit is prepared as the preceding, substituting for the *fucus vesiculosus*, the *chondrus crispus*, commonly called carrageen or Irish moss—an alga which is very abundant on the New England coast. It differs in appearance from the preceding, as also in taste. By some palates, it may, perhaps, be preferred. In nutritious and medicinal value there probably will be found little difference between them, the constitution of the two algæ being essentially similar.

ALGA CHOCOLATE

Is made by combining either of the marine algæ with cocoa. This alga chocolate is intended to replace cod-liver oil. This latter substance, whose utility in pulmonary disease is undisputed, is unfortunately very disagreeable to most tastes, and offensive to most stomachs. Whether it derives its medicinal value from the circumstance that, as an oil, it is a highly hydrogenous article of diet, or from its containing a small quantity of iodine, has not been decided, though probably both are concerned in the effect. It is claimed for the alga chocolate that, apart from its forming an agreeable and nutritious beverage, it should possess, in an eminent degree, all the virtues to be found in the cod-liver oil. Cocoa is known to contain fully one-half its weight of pure, oleaginous matter, which is sometimes

extracted by pressure and used in the arts under the name of cocoa-butter. It also contains a peculiar principle, known as theobromine, which is, perhaps, the most nutritious article derived from the vegetable kingdom, and enjoys the singular property, which has been fully recognized by physiologists, of preventing, to a great extent, in those who use it, the waste of the animal tissues. The algæ, associated with a body of such a constitution as that of cocoa, would seem at least from an *a priori* point of view, to form a compound well adapted to all such cases as would be benefited by cod-liver oil.

The Section have been satisfied, from experiments made before them, at their request, by Professor Ellet, that these preparations have not lost, in their manufacture, the leading medicinal agent—iodine—the presence of that substance in them having been abundantly demonstrated by the usual chemical tests.

It is due the Academy of Medicine to say that the idea of combining the marine algæ with other and more common articles of diet, is not now presented to the world for the first time, the suggestion having been made some years since, as reported in the second volume of the last (Philadelphia) edition of *Pereira*, from a Danish materia medica. The brief allusion there made to it, however (but which the Messrs. Coddington appear to have been entirely unaware of), makes no reference to the value of these combinations as vehicles of iodine—speaking of them only as articles of nutriment—and of one variety only of the algæ, the chondrus crispus, or carrageen.

The Section deem this effort of Messrs. Coddington to present the valetudinarian public, and the profession, this means of administering this valuable article of the materia medica, worthy of the commendation of the Academy.

On behalf of the Section,

John H. Griscom, M.D., *Chairman.*
John R. Van Kleek, M.D., *Secretary.*

New York, December 3, 1856.

TRANSACTIONS.

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RESEARCHES

INTO THE

STRUCTURE AND PHYSIOLOGY OF THE KIDNEY.

By C. E. Isaacs, M.D.,

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READ MARCH 5, 1856.

Within a few years past, great progress has been made in the pathology of the kidney, and it is therefore much to be desired that our knowledge of its anatomy should be as complete as possible, since every scientific examination of the diseased organ, or even of its secretion, is based upon our knowledge of the healthy structure, and must ever refer to this as its standard. We owe much to the researches of Malpighi and of Laurentius Bellini, who first discovered the tubular structure of the kidney, but we are greatly indebted to Bowman, Toynbee, Johnson, Henle, Gerlach, Bidder, Kölliker, and other living authors, for our present knowledge of the subject. I was induced to enter upon a long-extended investigation of the minute structure of the kidney, from the fact that the highest authorities were then, and are still at variance, with regard to some of the most important points in the anatomy of this organ. Thus the connection of the Malpighian body with the uriniferous tube, so strongly maintained by Bowman, is denied *in toto* by Huschke, Müller, and Hyrtl, of Vienna.

The existence of ciliated epithelium in the uriniferous tubes of the higher animals, of nucleated cells upon the Malpighian coil or tuft, of a fibrous matrix, and the arrangement of the

venous plexus, &c., are subjects upon which much difference of opinion exists among anatomists. It was evidently necessary that these points should be determined before we could arrive at a satisfactory physiology and pathology of the kidney. I was next led to inquire, why so many excellent observers had obtained such entirely different results, and the explanation seemed to be, that their means of investigation were insufficient. Thus some, as Bowman, had relied chiefly upon the appearances obtained by injection, while others had trusted almost entirely to those derived from specimens viewed as transparent objects under the microscope. Bowman, although sometimes employing the latter method, obtained nearly all his plates (except those which are diagrammatic) from specimens procured by the process of injection.

Toynbee, by the same method, arrived at entirely different results. It seemed, then, to me, that some other means of investigation were desirable, and especially some method by which the substance of the kidney could be rendered transparent; and that if this could be effected, it might be ascertained whose views (if any) were correct, and the true structure would probably then be determined. Accordingly, after numerous experiments with various chemical reagents, I at length arrived at the knowledge of certain processes, which have not only been useful by giving transparency to small portions and thin sections of the organ, but have often enabled them to be viewed both as opaque and as transparent objects.

In this manner, the observations have been rendered much less liable to error. I have not, however, limited myself to the use of these processes, but have also employed all the usual means, as injections, &c., &c. With all these aids, then, and without any preconceived theory, I entered upon an extensive and long-continued series of observations, and have given plates of the structures, as they were seen in the field of the microscope. I do not propose, in this paper, to enter upon all the details of the minute structure of the kidney, but at present shall merely refer to some points, to which it seems necessary at least to allude, in order that the subsequent remarks should be clearly comprehended.

The structure of the kidney is essentially tubular, each kidney, according to Huschke, containing more than two millions of minute tubes. These tubes are convoluted in the cortical portion, but straight in the pyramidal, and are each composed of a highly elastic, structureless, transparent, "basement" membrane. The integrity of the tube being of the highest importance during life, this membrane has been endowed with the quality of strongly resisting injurious influences, and even powerful chemical reagents. In virtue of this last-named property, we are enabled to show clearly the tubes of the kidney, by certain processes hereafter to be described. All the uriniferous tubes are lined internally by nucleated epithelial cells, by means of which the urine is separated, as it is believed, from the blood of the surrounding capillaries. The epithelial cells of the convoluted are generally thicker than those of the straight tubes.

Plate 1 exhibits the epithelium of the uriniferous tubes of the healthy human kidney.

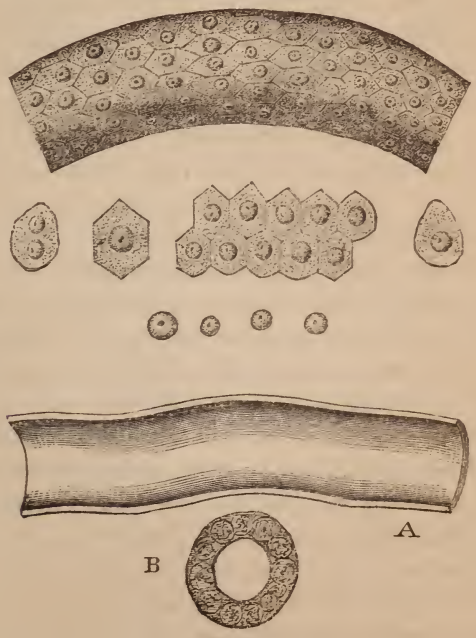

Plate 1.

The first figure in Plate 1 exhibits a convoluted tube lined by nucleated epithelial cells. The figure immediately below this shows the manner in which the cells, most of which are polygonal, are arranged. To the right of these is seen an oval-shaped cell; to the left, a cell containing two nuclei; and next to this is a polygonal cell, more highly magnified. Four separate nuclei of cells are also seen. A represents a uriniferous tube, the epithelium of which has been washed away, leaving only its basement membrane. This figure is diagrammatic. B is a transverse ring-like section of a uriniferous tube, showing its central canal, surrounded by epithelial cells.

In Plate 2, A exhibits one of the straight tubes in the pyramidal portion of the kidney. It is lined by polygonal, rounded,

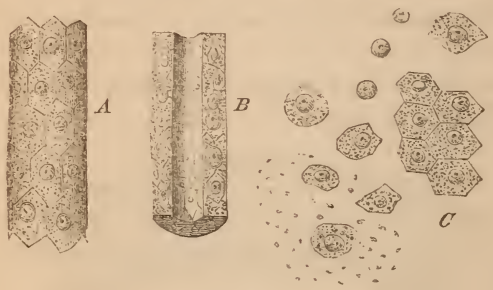

Plate 2.

and oval nucleated cells. B shows the central canal in the interior of a straight tube. C exhibits several polygonal cells. Some oval, rounded, and irregular cells, and a few separate nuclei and granules can also be perceived. Magnified 400 diameters. Most of the epithelial cells are polygonal, although many are oval, and others of a rounded or irregularly-rounded shape. They all contain finely granular matter, and a nucleus, which is itself composed of granules, among which one or two nucleoli can generally be observed. The contents of the cells are believed by Kölliker to contain albumen; he considers the granules to be also a protein substance. The epithelium very soon becomes changed from decomposition, or by the action of water, which expands the cells, and sometimes causes them to

burst, when the tubes are found to contain merely nuclei and granular matter. In examining the epithelium, *it is therefore very important* to obtain the kidney in as fresh condition as possible, and instead of water, to use a solution of albumen in water or urine. The acids dissolve the wall of the cells, leaving only the nuclei and granules; the nuclei finally disappear. By the action of caustic alkalies, everything in the cell is dissolved except the granules.

According to some very high authorities, the tubes of the cortical portion of the kidney are lined by the spheroidal or "glandular" epithelium. See Mr. Bowman's paper; also G. Johnson, on Diseases of the Kidney, pages 33 and 56. But in plates No. 1 and 2, it is shown that the uriniferous tubes are lined by the pavement or tesselated epithelium; and I find that, on this point, I am supported by Hassal, who says: "The epithelium of the tubes of the cortical part of the kidney, save within a short distance of their junction with the Malpighian dilatations, is composed of large and angular scales or cells, which are coarsely granular, and which form a regular layer of pavement epithelium, lining the tubes," &c.—*Micros. Anat.*, p. 444. Kölliker says, "The tubuli uriniferi are every where composed of the same elements, viz., a membrana propria, and a tesselated epithelium." He also remarks, "a single layer of *polygonal*, moderately thick cells, surrounds the cavity of the tubulus," &c. —*Micros. Anat.*, p. 599.

I believe that the description which is given in this paper will always be verified by actual observation of specimens of the kidney, provided they are in a perfectly fresh and healthy condition. It is, however, extremely difficult to meet with specimens of the kidney which are sufficiently so to exhibit the perfectly normal epithelium, and I therefore made every exertion to obtain the organs in a satisfactory condition, for this purpose. I am well aware that we often see the tubes containing rounded, and irregularly rounded, epithelium, as described by authors; but I believe that when this appearance is seen, the kidney is either not fresh, or is in a pathological condition, or that the epithelium has been changed by the mode of examination. Kölliker says: "By the usual method of exami-

nation in water, the cells expand, owing to its absorption, and become vesicularly distended, so that their polygonal form and regular arrangement are lost, the renal ducts appearing to be filled with larger rounded cells, and no longer to possess any cavity. In kidneys not quite fresh, these changes proceed spontaneously."—*Micros. Anat.*, p. 599.

In urinary diseases, where the epithelium is thrown off, either as loose cells, or in the form of casts, these cells are generally observed of a more or less rounded form, with an unusual amount of granular matter between and among them, arising from their disintegration. By most physiologists, it is considered that the epithelial cells are concerned in separating from the blood all the proximate elements of the urine, and they believe that this idea is confirmed by the fact that these cells are of the rounded, spheroidal, or "*glandular*" variety.

How far these cells are concerned in the secretion of the urine, will be hereafter considered. For the present, it may be said, that the rounded form would not appear to be necessary in order that they might effect this secretion, inasmuch as the hepatic cells, which are concerned in the separation or secretion of bile, are of the tesselated variety, and of an irregular polygonal form.—See Kölliker, *Micros. Anat.*, p. 532. He says, "The hepatic cells resemble tesselated epithelium cells in form, except that they are more irregular." It will be remembered, that of all secretions, the bile is the most complex. The epithelial cells are liable to many important pathological changes, to which the limits of this paper will merely permit an allusion. They sometimes contain, or are mixed with, blood, pus, oil globules, crystals of lithic, or oxalic acid, &c. The cells themselves may be thrown off as a fibrinous cast, or they may become disintegrated or broken down, and obstruct the tube, thus interfering with the excretion and secretion of the urine, and consequently with the circulation, and nutrition of the kidney. See G. Johnson's excellent work on the Kidney, and other recent authors on its diseases. In fishes, frogs, snakes, turtles, &c., the uriniferous tubes are lined by ciliated epithelial cells, by the action of which the current of urine in the tube is directed and urged onward towards the ureter.

Authorities, however, are not agreed as to its existence in the mammalia.

Hassal says that this has been doubted or denied by Huschke, Reichert and Bidder, while Bischoff, Valentin, Pappenheim, Gerlach, and Kölliker are of the contrary and affirmative opinion, and that he has himself seen ciliary motion in the kidney of the sheep, rabbit and horse. During the last three or four years, I had occasionally examined the kidney in the higher animals, as soon as possible after death, with the view of detecting ciliary motion. After many examinations, having uniformly failed in obtaining any evidence of its presence, I had become much inclined to doubt its existence; but being desirous of testing the question more extensively, I resorted to the large establishments, in this city, for killing oxen, sheep, dogs, rats, &c., and examined the kidneys immediately after the death of the animal. I may here mention, that every precaution was taken not to confound molecular with ciliary motion, as has probably been done by some observers. Some of the scraped substance of the kidney was gently agitated, in a test tube, containing a solution of albumen, and a drop of this fluid was then placed under the microscope. Thin sections were also used. In some animals no motion could be perceived, but in the dog I observed currents taking place, in the fluid, and also within the uriniferous tubes. The epithelial cells would frequently disengage themselves from the sides of the tube, and pass along for a considerable distance, and after emerging from the mouth of the tube, would assume a rotatory motion. Sometimes nearly all the epithelial cells would pass out of a tube, in the space of fifteen or twenty minutes, leaving it almost denuded of its internal epithelial lining. I have also seen isolated cells, having a vibratory or rotatory movement. These appearances I have noticed, upon eight occasions, in the kidneys of dogs. Such motions generally cease, in these animals, in less than an hour after death; but, at one time, I noticed their continuance for more than three hours after the animal was killed. I have often seen appearances similar to the preceding in the kidneys of the ox and sheep. Nevertheless, it must be stated, that after very numerous

and careful observations, I have never seen the epithelial cells actually provided with cilia, except upon one occasion, when I observed a single cell apparently fringed with cilia, and in active rotatory motion. This was in the kidney of the ox. In these examinations, I made use of various magnifying powers, and especially the Nos. 5 and 7 objectives of Natchet, and the $\frac{1}{4}$ objective of Powell & Lealand, but the result has been as above stated. In examining minute portions of the oyster and clam, which possess ciliary motion, we sometimes observe particles of a rounded or irregular shape, and which cause currents in the fluid in which they float, but we cannot always discern that these particles have cilia attached to them, although they may have a rapid, and apparently an independent rotatory or vibratory motion. What is observed in the kidneys of the dog, sheep, and ox, certainly seems to be much more powerful, and different in its nature from molecular motion. Reasoning from the appearances in the oyster and clam, as well as from analogy in many of the lower animals, it may be concluded that ciliary motion does exist in those of a higher grade, although it is, in them, very imperfect, or, as it might perhaps be said, in a rudimentary condition.

In some of the inferior animals, where the urine is excreted in the semi-fluid state, a much greater necessity exists that this fluid should be rapidly propelled in its course through the uriniferous tubes, and, accordingly, we here find ciliary motion in its most perfect condition. I have never seen it in the human kidney, even after many careful examinations. If it exists, which is probable, it ceases very soon after death, when it rarely happens that we can obtain an opportunity of examining it.

The uriniferous tubes of the kidney present an immense internal secreting surface, which, according to Valentin, is almost six times as large as the whole surface of the skin. The substance of the kidney is very vascular; most, *but not all* of the terminal twigs of the renal artery, in the cortical portion of the kidney, ending in the Malpighian coil or tuft of capillaries, while the veins returning the blood from this coil enter into the capillary venous plexus, through whose meshes the convoluted tubes pursue their tortuous course.

In Plate 3, A is a small branch of the renal artery; B, a smaller branch, which divides into two twigs, one of which

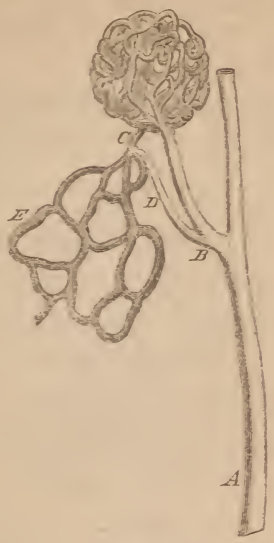

Plate 3.

supports at its extremity the Malpighian coil or tuft of capillaries; the other enters into the venous plexus, marked E. C, a vein which returns the blood of the Malpighian coil into the venous plexus, E. D, an arterial twig, communicating with the venous plexus. Magnified 130 diameters.

In reference to this point, Mr. Toynbee says: "The single trunk which emerges from the corpus (Malpighianum) sometimes unites with another, proceeding from an arterial branch, which is entirely unconnected with the corpora." Since writing the preceding remarks, I have found a paper upon the Anatomy and Physiology of the Kidneys, by Dr. R. McDonnell, published in the Glasgow Medical Journal, October 1854. Speaking of the branches of the renal artery, he says: "They give off some small twigs, which furnish capillaries to the straight urinary tubules, and are ultimately lost on the boundary between the medullary and cortical portions, in small vessels, *of which only a small part passes into the capillary system of the cortex*, while by far the greatest part passes on to,

and pervades the Malpighian capsules." It will thus be seen that the venous plexus contains a certain amount of arterial blood, derived from those minute twigs of the renal artery, in the cortical portion of the kidney, which do not enter the Malpighian capsules. (See Plate 3.)

Plate 4 shows the injected plexus of veins on the surface of the kidney of the sheep, and the spaces which exist in this

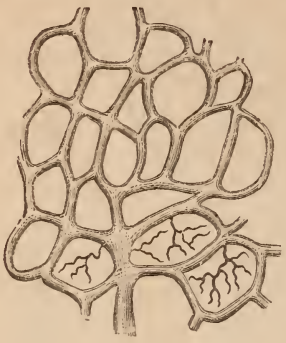

Plate 4.

venous network. In these spaces the so-called lobules of the kidney are contained. Three of the spaces show small venous branches, which run around and among the tubes on the surface of the organ. Viewed as an *opaque* object, and magnified 40 diameters.

Plate 5 shows a portion of the above specimen made trans-

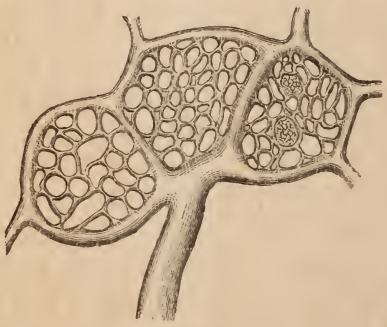

Plate 5.

parent by chloroform, and viewed by transmitted light. Mag-

nified 80 diameters. The venous spaces are seen to be occupied by tubes. The dark line around each tube is a capillary vein. In one of the spaces, two Malpighian bodies can be seen, surrounded by tubes.

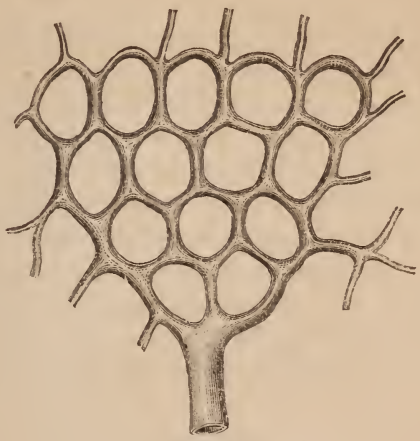

Plate 6.

Plate 6 shows a part of one of the three venous spaces in

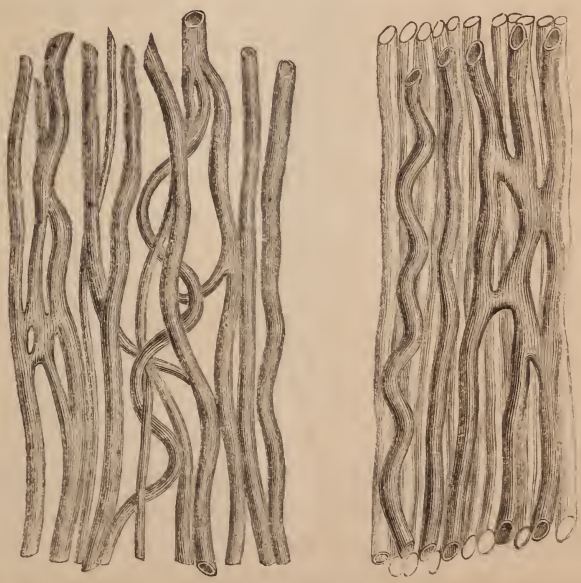

Plate 7.

Plate 5, but much more highly magnified; viz., 130 diameters. A venous trunk is here seen to form a mesh or network of capillary veins, which surrounds the uriniferous tubes. Rendered transparent by chloroform, and viewed by transmitted light.

Plate 7 exhibits a small part of a longitudinal section of the pyramidal portion of the kidney. The specimen was injected from the vein, made transparent by chloroform, and could be viewed either as an opaque or as a transparent object. This plate was drawn from the specimen while in the latter state. Magnified 90 diameters. The figure on the left shows the veins of the pyramids. On the right, veins lying upon and amidst the straight tubes. So numerous were they, that when viewed as an opaque object, the pyramid seemed to consist entirely of veins.

Plate 8 shows the straight tubes. The tubes are drawn as having their cut extremities open. From the minute venous

Plate 8.

network on the surface of the kidney, which often exhibits a stellated appearance, small veins are formed, which descend, or pass inwards, between the fasciculi of the tubes, gradually increasing in size by receiving numerous branches from the

interior of the cortical portion. The venous trunks, which have thus been formed, join the veins which lie around the periphery of the pyramids, and which return the blood of those bodies. By these means, a venous trunk of considerable size is formed, which unites with others (originating in a similar manner) to constitute the branches of the renal vein. The general arrangement of the minute arteries, the Malpighian bodies attached to them, and that of the venous plexus, may be shown by successful injections. By this process the tubes may sometimes be very beautifully shown, but their peculiar connection with the Malpighian coil, or tuft of capillaries, their basement membrane, and its epithelial lining, and the cellular framework, or matrix of the kidney, can only be clearly and satisfactorily exhibited when these parts are viewed as transparent objects under the microscope. To view the tubes of the kidney in their normal condition, very thin sections or scrapings of the cut surface of the organ may be put into a test-tube with water, agitated for a few minutes, placed on a slide of glass, covered by a thin slip, and then examined under the microscope. The most distinct and beautiful views of the form and arrangement of the Malpighian bodies and tubes of the kidney can be obtained by the process about to be described, which enables us to study thin sections of the substance of the kidney as transparent objects. Occasionally the connection of the tube with the Malpighian body can be seen, as well as the attachment of the latter to the terminal arterial twig. Small pieces of the organ are put into a test-tube, with about half an ounce of water, to which three drops of pure sulphuric acid are then added, when it is boiled for one or two minutes. A thin section being now placed under the microscope, the tubes will be most clearly and distinctly exhibited. If too much acid be used, it will dissolve all the minute vessels and capillaries, but not the Malpighian tuft or coil. This is worthy of notice, as showing a great difference in the chemical constitution, and consequently in the organization, of these different parts.

Plate 9 shows the convoluted tubes A, passing into straight tubes B. C is a Malpighian body, connected with a convoluted uriniferous tube. This tube is seen to pass downwards, and

become continuous, with a straight one. D a Malpighian body, with the uriniferous tube connected with it. Magnified 80 diameters. I have succeeded in exhibiting the tubes of the kidney,

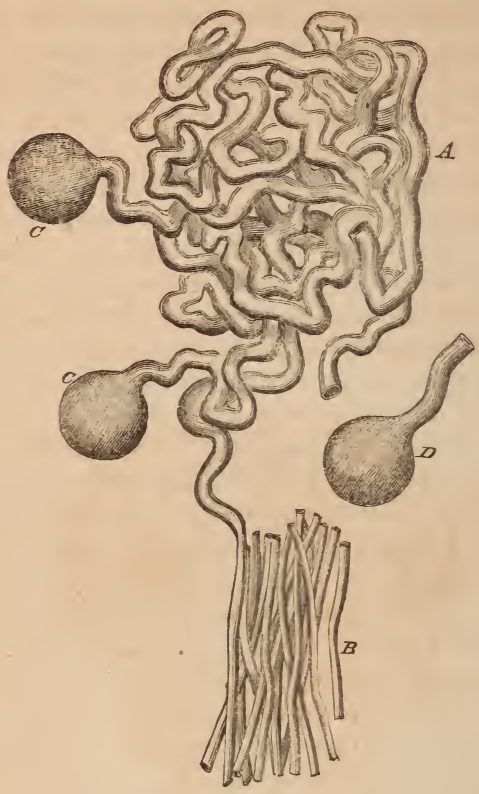

Plate 9.

very distinctly, by boiling small pieces of the organ in diluted chloroform, and also in solution of chlorate of potassa, which last is useful in giving a clear view of the surface of the kidney when congested, as it shows the venous plexus and the tubes, and acts but slowly upon the blood globules. Muriatic, acetic, and nitric acids, also exhibit the tubuli with considerable distinctness; the last however, is apt to discolor the tubes. I have also tried the effect of many other chemical reagents, among others, the phosphoric, chromic, boracic, tartaric, and citric acids, the

alkalies, and their carbonates, various salts, &c. Some of these chemical agents act strongly upon the blood vessels, and are apt to destroy them, unless used in a very diluted state. As, however, it was very desirable to show the vessels in connection with the Malpighian bodies, and, at the same time, to exhibit the tubes, after some reflection, I adopted the following plan:—The vessels were injected with white lead, finely ground in oil, (artist's tubes) and well agitated with sulphuric ether. This material for injection was recommended some years ago by Dr. Goddard, of Philadelphia. After injecting the kidney, small pieces of the organ were boiled in very diluted chloroform. Some thin sections were made with Valentin's knife, and then placed under the microscope. Other sections were first dried, then immersed in spirits of turpentine, and finally placed on a glass slide, in a drop of water, under the microscope.

Plate 10.

Plate 10 exhibits a portion of the kidney of the sheep, prepared as above.

The Malpighian bodies, and the small arterial twigs, upon which they are placed, are seen lying among, or in the midst the convoluted tubes. The capillary venous plexus is also seen upon, and in the midst of the tubes, or the tubes may be said to lie in a mesh or network of this plexus. Magnified 80 diameters.

Plate 11 exhibits a thin section of the kidney of the sheep, also injected with white lead.

Plate 11.

This was then boiled, with two drops of sulphuric acid, in three drachms of water, washed repeatedly, and thin slices then placed under the microscope.

This Plate shows the Malpighian bodies lying among or amidst the convoluted tubes. The small veins which return the blood from the Malpighian bodies into the venous plexus, and the plexus itself, are also seen. Magnified 80 diameters. The epithelium of the tubes and their basement membrane, may be seen by the process already described, but inasmuch as water has a tendency to break down the epithelial cells, I resorted

to the use of a solution of albumen in fresh urine. Thin sections, or scrapings of the kidney, may be agitated in this fluid, and then placed under the miscroscope.

One of the most difficult questions in the minute anatomy of the kidney is whether any connection exists between the Malpighian body and the uriniferous tube, and if so, what is the true nature of that connection? Having been much disappointed in my repeated attempts to ascertain the truth in this matter, I adopted the following processes:—Fine scrapings of the kidney may be agitated with water in a test tube, and then examined microscopically, or they may be boiled in half an ounce of water, to which one drop of sulphuric acid has been added. The precipitate is to be repeatedly washed, and then examined as above.

Plate 12.

Plate 12 is a view obtained by agitating scrapings of the kidney of the sheep, (which had been previously injected with chrome yellow and sulphuric ether) in a test tube, with water. Among the minute fragments was one, from which the above drawing was taken. A small artery is here seen, dividing into two branches, each supporting a Malpighian tuft, or coil of capillaries. To one of these is attached the expanded extremity of the convoluted uriniferous tube. Magnified 130 diameters.

Plate 13 shows a minute artery, passing to the expanded

extremity, or capsule of the uriniferous tube. The artery has been filled, and the Malpighian coil, or tuft of capillaries, has been ruptured by the injected material, which has thus passed

Plate 13.

into the uriniferous tube. From the kidney of the deer. Magnified 80 diameters. Drawn by Dr. Armenius Oemler.

Plate 14 exhibits the uriniferous tube and capsule, injected from the ureter, with white lead. From the kidney of the moose. Magnified 80 diameters. I have also succeeded in showing the connection of the tubes with the Malpighian body, by suspending small pieces of the kidney in the vapor of alcohol for two or three weeks, and then placing very thin sections of the organ in water, in a test tube, and agitating occasionally for 24 or 36 hours, and then subjecting very small portions to

Plate 14.

the microscope. In this manner, I have obtained some exceedingly clear and beautiful views.

Plate 15.

Plate 15 exhibits the above named parts from the kidney of the cat, obtained by the process just described.

A and B represent the Malpighian bodies. Nucleated epithelial cells are seen, which line the interior of the capsule, and are also placed upon the Malpighian coil, or tuft of capillaries. The tubes also contain epithelium, and some oil globules, which are almost always to be found in the kidney of cats.

The wall of the tube, or basement membrane, can be traced up to the point where it expands to form the capsule, or covering of the Malpighian coil or tuft of capillaries. The tuft, covered by the capsule, constitutes the Malpighian body. Magnified 250 diameters. A process, however, which I would particularly recommend, is the following. Put into a test tube half an ounce of water, and add to it two or three drops of chloroform; a small portion of the fine scrapings of the kidney (about the size of a large pistol ball) is then to be dropped into the fluid, and the tube, with its contents, heated over a spirit lamp, but not sufficiently to cause boiling; the fluid is to be poured off from the deposit, which is to be carefully and repeatedly washed with pure water, and small portions are then to be placed under the microscope. The following view was obtained by this process from the kidney of the ox.

Plate 16.

Plate 16 exhibits a Malpighian body, and its connection with the uriniferous tube. The smaller object is magnified 50 diameters. The same object, on the left hand, magnified 80 diameters. Epithelial cells are seen through the capsule of the Malpighian tuft, and within the uriniferous tubes.

Plate 17 exhibits a thin section of the kidney of the sheep, obtained also by the process just described. Magnified 40 diameters. Drawn by Dr. Armenius Oemler. A exhibits the

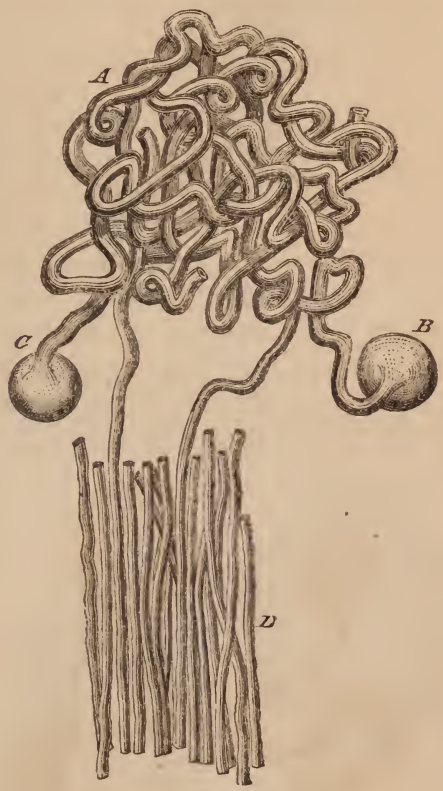

Plate 17.

convoluted tubes, passing down to become continuous with the straight tubes. B, C, and D, show the connection of the Malpighian bodies with the convoluted tubes.

Plate 18 shows the connection of the Malpighian body with

the uriniferous tube. From the kidney of a cat, in which the right renal vein had been tied, and the ureter of the left kidney

Plate 18.

also ligated. The animal died in fifty-six hours after the operation, stupor and increasing coma having supervened some hours before death. The specimen appears dark, from the blood in the tubes and Malpighian body.

Plate 19.

Plate 19 is a view of the Malpighian body, and its connection with the uriniferous tube. Epithelial cells are seen within the tube, and through the capsule of the Malpighian tuft or coil. This view was obtained by agitating scrapings of the kidney in a solution of albumen in urine. From the kidney of the sheep. Magnified 400 diameters.

Plate 20.

Plate 20 represents the Malpighian body and uriniferous

Plate 21.

tube connected with it. Obtained by agitating scrapings of the organ, with water, in a test-tube. From the kidney of the gray squirrel. Magnified 80 diameters. These parts appear to be smaller in the squirrel than in any of the other animals to which reference has been made.

Plate 21 exhibits the connection of the convoluted uriniferous tube with the Malpighian body, in the kidney of the common domestic fowl. The tube, where it passes up to form the capsule, or covering for the Malpighian tuft, does not pre-

Plate 22.

sent the constriction usually seen in the kidneys of the mammalia. Magnified 250 diameters.

Plate 22 also shows the connection of the uriniferous tube

Plate 23.

with the Malpighian body. From the kidney of a large snake, obtained from the river Amazon. Magnified 400 diameters.

Plate 23 likewise exhibits the connection of the Malpighian body with the uriniferous tube. From the kidney of a large fish (haddock). Magnified 400 diameters.

Conclusion as to the connection of the Malpighian tuft, or coil of capillaries with the convoluted uriniferous tube, and a statement of the opinions of authors upon this subject.

By the employment of the various processes, which I have described, even at the risk of being considered as tedious, because they enable us to observe, with comparatively greater clearness and facility, what otherwise cannot be seen without great difficulty and much loss of time, I have satisfied myself, beyond the slightest doubt, by very many and repeated observations, conducted almost daily for the last twelve months:

1. That the convoluted uriniferous tubes terminate by forming an expanded extremity or capsule, which embraces the Malpighian tuft, or coil of capillaries, as was first correctly described by Mr. Bowman, in his excellent paper upon the kidney. I am, however, well aware that very different views upon this point have been held by Toynbee, Müller, Gerlach, Bidder, and other authors.

Some indeed have even denied that any connection existed between the Malpighian body and the uriniferous tube. Thus Hüschke says: "These corpuscles (the Malpighian) are without any connection with the uriniferous ducts." Müller observes: "Fines ductuum uriniferorum in corpora Malpighiana desinere, certissime falsa assertio est." And again, he says: "Falsissima est opinio de connexu ullo quopiam inter corpora Malpighiana sanguifera et ductuum uriniferorum fines."

In a paper published in the *London Medical Times*, April, 1846, Professor Hyrtl, of Vienna, maintains that "the Malpighian capsules have no connection with the urinary tubules, but open into the lymphatics."—(*British and Foreign Medical Review*, 1846.) Mr. Toynbee, in his plates of injected kidneys,

represents the uriniferous tube as penetrating the capsule of the Malpighian tuft, then twisting into a coil within the capsule, and finally emerging from the capsule. Gerlach says that the Malpighian capsule is not a blind termination of a uriniferous duct, but, on the contrary, a retraction, introversion, or diverticulum of the same membrane which forms the urine tubes. He, however, correctly describes the Malpighian tuft as covered by nucleated cells. (See his Beiträge zur Structurlehre der Niere, in Müller Archiv., 1845.) According to Bidder, the Malpighian tuft or coil is pushed into an expanded portion of the uriniferous tube, the coil being *external* to the cavity of the dilated extremity of the tube, the relation of the two being similar to that of the head within a double nightcap. (See Bidder, in Müller Archiv., 1845.)

This difference of opinion, even among such high authorities, may probably be accounted for, inasmuch as it is very difficult, by employing the usual means of examination, to obtain any minute portion of the organ which will show the tube connected to the Malpighian body. Moreover, in examining any thin section of the kidney (as is usually done) for this purpose, it is to be remembered that the Malpighian body is always embraced in a ring of the fibrous matrix, and that at the neck of the tube, or its commencement of expansion into the capsule, is its weakest part, and where it is most easily, and indeed almost always, torn across, especially when traction is made upon it, as is usually done, in tearing out the specimen with needles. On the contrary, by using *scrapings* of the organ (as here recommended) agitated in water, which softens and removes many of the adhering portions of the matrix, which last holds and confines the Malpighian body, this can be washed out, and not unfrequently with the convoluted tube attached to it. (See Plates 15, 16, 19, 20, 21, 22.)

I may now remark, that having had no particular theory to influence my investigations, my sole object has been to ascertain what was the real structure, and to represent accurately what I have so frequently observed. I refer, also, with much satisfaction, to the testimony of others, to whom I showed the specimens, under the microscope, and particularly to Drs. J. P.

Batchelder, J. M. Minor, formerly Surgeon of the U. S. Navy, Professors J. T. Metcalfe, George T. Elliot, and Alfred C. Post, Drs. Stephen Smith, J. S. Goulay, John T. McNulty, George A. Peters, T. C. Finnell, and many others, who were satisfied that the structure was strictly that which has been represented in the plates here given. Within the last week, I have met with a paper on the structure of the kidney, in some of the mollusca, and in birds and serpents, by Dr. Busch, of Berlin, in the last August number of Müller's Archiv, for 1855. He says:—"My experiments on snakes have fully convinced me that the glomerulus (or Malpighian coil) is truly situated in the expanded end of a uriniferous tube." "Dass der glomerulus wirklich, in einer kapsel, dem erweiterten ende, eines Harn canälchens, gelegen sei."

His description is very similar to that of Bowman, but he differs from him in representing the Malpighian tuft as covered by nucleated cells. A plate of the structure in the snake is also given.

On the termination of the convoluted uriniferous tubes.

The convoluted tubes form loops, and also become continuous with the straight tubes, but I have never seen blind extremities, or the anastomoses of the tubes, as described and figured by Toynbee and others, except in the kidney of frogs, fish, and turtles. I would not wish, however, to be understood as denying their existence.

On the question whether nucleated cells exist upon the Malpighian tuft or coil of capillaries.

Much difference of opinion on this point still prevails among anatomists, and, as will be seen hereafter, the determination of this subject is very important in reference to the physiology of the kidney. The epithelium of the tuft is, perhaps, best seen in the kidney of frogs, snakes, etc., but I am not aware that any anatomist has satisfactorily demonstrated it in that of the higher animals.

In the investigation of this subject, it seemed to be very difficult to ascertain the true arrangement, and for the following reasons:—In looking at the Malpighian body, which consists of the tuft or coil of capillaries, and the expanded extremity of the uriniferous tube, or the capsule, as it is called, we observe a number of cells, which seem to lie on the surface of the Malpighian body, but which are really on the inner surface of the capsule. See Plate 19. As the capsule embraces the tuft or coil, and ordinarily is closely applied to it, it results that the cells lining the interior of the capsule must conceal those which are on the tuft, if indeed such there exist.

It would thus seem almost impossible, in looking at a Malpighian body, to decide whether the cells which we see *apparently* upon its surface, are really those on the inside of and lining the capsule, or whether they are on the tuft, and are perceived through the transparent capsule, or, lastly, are they the cells, both of the tuft and of the interior of the capsule?

After much reflection as to the best plan of determining this point, I adopted the following processes:—

1. By injecting watery and etherial solutions into the ureter, I succeeded in bursting the capsule, the Malpighian tuft or coil having been previously only slightly, and as was intended imperfectly injected from the artery. Epithelial cells could, then be seen upon the uninjected and transparent edges of the tuft or coil.

Plate 24, Fig. 1, exhibits a Malpighian tuft. Broken fragments of the injected vessels are seen within it. They had been injected with chrome yellow, and appear black when the specimen is viewed by transmitted light. The uriniferous tube had been distended by the injection from the ureter, and its expanded extremity, or capsule, had been burst, and can be perceived lying in shreds at the sides of the tuft, which is now uncovered by the capsule. Nucleated cells can be seen upon the naked and uninjected parts of the tuft.

From the kidney of the black bear. Magnified 80 diameters.

Fig. 2. In this specimen the capsule had been scratched off with a needle, and then the naked tuft somewhat torn under

the microscope. In Fig. 3, some small fragments of the tuft are here seen with nucleated cells on their surface.

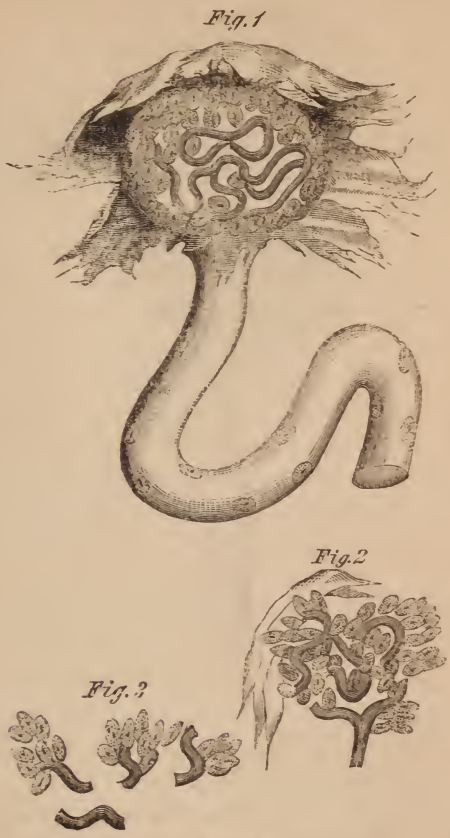

Plate 24.

4. The capsule was torn off from a Malpighian body with a needle. In doing this, the capsule became reversed, so as to give a view of its internal surface, upon which small nucleated cells could be clearly and distinctly seen. The surface of the naked tuft was covered by cells of much larger size than those upon the interior of the capsule. Upon the application of dilute nitric acid, the wall of the cells of the capsule was dissolved, while comparatively little effect was produced upon those of the tuft, thus showing a difference in their chemical

constitution and organization. In Plate 25, A is the uriniferous tube; B, cells on the tuft; C, reversed and inner surface of the

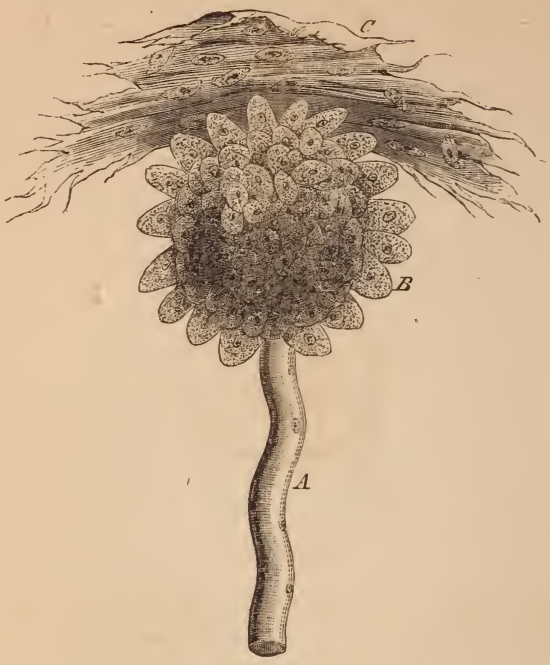

Plate 25.

capsule, covered by cells differing in their appearance and chemical reactions from those on the tuft. From the kidney of the raccoon. Magnified 400 diameters.

5. Fine scrapings of the kidney were agitated occasionally for two or three days, in a test tube, the water having been frequently changed. By this method, the epithelial cells within the capsule were washed out, so that the space thus left between the tuft and the capsule became filled with water, which had soaked through the capsule. See Plate 26. At A is seen the uriniferous tube. A few cells yet remain in its interior. B denotes the capsule. At C is perceived the minute arterial vessel which enters the tuft.

By slight agitation, while the specimen was floating in the water, under the microscope, it could be rolled over and over,

so as to show various points of the surface of the tuft, covered

Plate 26.

by nucleated cells. As it repeatedly turned over in the water, it resembled a ball, suspended in a transparent bag, or bladder of water, and slightly swaying from side to side. From the kidney of the cat. Magnified 250 diameters. By the different processes just mentioned, I consider the existence of nucleated cells upon the surface of the Malpighian tuft, and, consequently, its analogy with the other secreting organs, as conclusively demonstrated.

Conclusions as to the Functions of the Malpighian Body.

According to Mr. Bowman, the Malpighian coil or tuft of capillaries lies entirely *naked* in its capsule, and its office is simply that of separating water from the blood, while the urea, lithic acid, and salts, are separated by the epithelial cells of the tubes from the blood of the venous plexus which surrounds them. These views, and some others, of Mr. Bowman, which will soon be presented, have been so extensively adopted, and

copied into the text-books on physiology, that they deserve our most attentive consideration.

1. It is probable, that in other organs, water is never separated from the blood, simply as such, in any case whatever; but that it then always contains albuminous substances, and certain salts, and not unfrequently other materials. Nor do we find that a peculiar or special arrangement of vessels is required, even for the exhalation of serous fluids, but that, on the contrary, they may exude, or pass by simple exosmosis, through the minute vessels of any of the organs which are composed of soft and yielding tissues. Thus, the cerebro-spinal fluid permeates the vessels of the pia mater and the areolar tissue, and the serous membranes are moistened by a fluid, or, even in health, may contain a small quantity, exhaled from capillaries which are most simple in their arrangement. On the contrary, the Malpighian coil or tuft is so complex, and so peculiarly formed, that in man, and in the higher animals, we shall find that no organ except the kidney contains the same, or even a similar structure. It would, therefore, scarcely appear necessary that the Malpighian tuft, which is so complex in its organization, should be needed merely for the purpose of separating water from the blood, which could be effected by a much more simple arrangement of vessels, as well as by the entire and extensive epithelial surface of the uriniferous tubes.

2. The presence of peculiar nucleated cells upon the Malpighian coil or tuft, would seem to denote that some of the constituents of the urine, besides water, are secreted by them.

3. In birds and reptiles the urine is secreted either in the semi-fluid, or, as in the latter class, almost in the solid form.

Simon says: "The urine of serpents is excreted as a white, pultaceous, earthy mass, which soon stiffens on exposure to the air. It is composed, for the most part, of uric acid in combination with potassa, soda, and ammonia, together with phosphate of lime." (Simon, p. 562.) According to Prout, the urine of the boa-constrictor contains more than ninety per cent. of uric acid. The urine of the rattlesnake, analyzed by Simon, was composed almost entirely of uric acid and the urates. (See Simon, p. 54.) According to Jos. Jones, who has analyzed the

urine of several species of snakes, their urine contained uric acid, always in combination with ammonia; he could not detect the presence of urea. (See his very valuable paper in the *American Journal of Medical Sciences*, April 1855.) Yet in these animals the Malpighian bodies exist, not only in the cortical portion, but throughout the entire kidney; and if, as asserted, their function is merely that of separating water from the blood, this fluid should be in excess; but it appears, on the contrary, that the urine is excreted almost in the solid state.

4. Will the entrance of foreign substances into the circulation, and their detection in the kidney and in the urine, throw any light upon the function of the Malpighian coil or tuft of capillaries? Let us examine carefully the following facts. According to Simon, after administration of the following substances, they can be detected in the urine:—Iodine and bromine, in combination either with ammonium, sodium, or potassium. Sulphur, ferrocyanide of potassium, salts of arsenic, antimony, iron, mercury, nickel, gold, silver, tin, lead, bismuth, copper, and manganese, have all been found in the urine. This was also the case with the organic acids—as the tartaric and citric acids. Meconic acid has been detected in the urine of animals poisoned with opium, as have also morphine and quinine. Coloring and odorous, or volatile substances, have often been recognized in the urine, as indigo, gamboge, rhubarb, madder, Indian fig (which colors the urine blood-red), garlic, turpentine, cubebs, and balsam copaiba. Alcohol has very often been found in the urine. Lehman, after taking phloridzin, discovered hippuric acid and oxalate of lime in his urine. I have myself repeatedly swallowed solutions of the ferrocyanide of potassium, and in fifteen or twenty minutes ascertained its presence in the urine by its striking a blue color with the salts of iron. I have also frequently taken small quantities of benzoic acid, and found, on microscopic examination of the urine, that it had been changed and separated from the kidney under the form of crystals of hippuric acid.

Urea has been given internally by some of the French physiologists, and has acted as a powerful diuretic, and on such occasions has been found in large quantity in the urine. Now

then, it is admitted by Mr. Bowman that diuretics must enter the circulation and pass along with the blood of the renal artery into the Malpighian body. He even says: "Diuretics appear to act specially upon the Malpighian body, and various foreign substances, particularly salts, which when introduced into the blood, pass off by the urine with great freedom, and exude, in all probability, through this *bare* system of capillaries. The structure of the Malpighian body indicates this; the escape also of certain morbid products, occasionally found in the urine, seems to be from the Malpighian tufts. I allude especially to sugar, albumen, and the red particles of the blood; the two first of which would transude, while the last would escape only by rupture of vessels." (See Bowman, *Philosophical Transactions*, 1842.)

It is then conceded, and we have every reason to believe, that these foreign substances do actually pass through the renal artery, and consequently through the Malpighian tuft or coil; and if this tuft *can* separate all the various substances above enumerated as having been found in the urine, and especially the various salts and the urea, are we not justified in believing that in the performance of its normal function it does something more than merely separate the water from the blood?

The following is an interesting illustration of the capability of the Malpighian tuft for the separation or elimination of coloring matters. Within a few days past, my friend, Dr. J. C. Hutchison, of Brooklyn, has presented me with some specimens of diseased liver and kidneys taken from the body of a man who had been very intemperate for the last twenty years. The patient was deeply jaundiced for some time previous to death; the kidneys were slightly indurated. On making a microscopic examination of very thin sections of the kidney, I found that some of the convoluted uriniferous tubes contained particles of bile, and that some of the epithelial cells were stained of a deep yellow color by this substance. *The Malpighian bodies were of a slight yellow or brassy color.* On applying Pettenkoffer's test, the Malpighian bodies and the colored contents of the tubes showed a play of colors, and finally were reddened by the action of nitric acid. Thin sections of the

kidney, steeped in water, colored it slightly yellow; and this fluid, acted on by nitric and by muriatic acid, assumed the usual varying colors, thus fully demonstrating the presence of bile. These facts seem to prove that the Malpighian tuft has the power of separating the coloring matter of the bile, and it would be difficult to explain its presence in the urine except on the supposition that it had passed through the coil or tuft.

Lastly—Simon, Marchand, and many others have detected urea in the blood of healthy animals. Valentin says: "It may be recognized in the blood, in the saliva, in the aqueous and vitreous humors, in dropsical fluids," &c. In cases of Bright's disease, and especially after extirpation of the kidneys, it is found in the blood in large proportion. According to Dr. Garrod, uric acid also exists ready formed in the blood, even in health, and can always be easily detected in that of gouty patients.

The coloring matters of the urine, and the various salts, as the phosphates, sulphates, and chlorides, pre-exist in the blood. This is also the case with the kreatine and kreatinine, which are derived from the disintegration of the muscular tissues. It is, then, proved that all the elements of urine are first formed in the blood, and subsequently separated by the kidney. Thus the blood which contains them *must* pass through the renal artery and the arterial terminal twig which supports the Malpighian body. The blood passes slowly through this vascular coil or tuft, and its velocity is also retarded in consequence of the small size of the "vas efferens," or the vein which returns the blood of the tuft into the venous plexus. (See Plate 10.) Here also the cells which lie immediately upon the tuft aid in the separation of the elements of the urine. (See Plate 25.)

If any constituent of the urine should escape separation from the blood while in the Malpighian body, it must then pass through the vas efferens into the venous plexus, the arrangement and function of which will now be considered.

It has been already stated, that according to Mr. Bowman, whose views are entitled to the highest respect, and have been almost generally adopted, the urea, lithic acid, and salts

are separated by the epithelial lining of the tubes from the blood of the venous plexus which surrounds them. The minute vein which returns the blood of the Malpighian tuft or coil, or the "vas efferens," as it has been termed, is similar in its nature to the vena porta, and the blood of the venous plexus to that of portal blood; or, as he observes, "each efferent vessel is a portal vein in miniature. In support of his theory, he states that, "in serpents the kidney *receives* not only a renal artery, but also a '*large renal portal vein*,' bringing, for the secretion of urine, the venous blood of the hinder parts of the body, and giving off the capillaries which ramify upon the urine tubes." —(Bowman, *Philosophical Transactions*, 1842. See Plate 27, which is a view of the minute structure of the kidney in the boa-constrictor.)

In Plate 27 *A* is a small branch of the renal artery; *Af* a

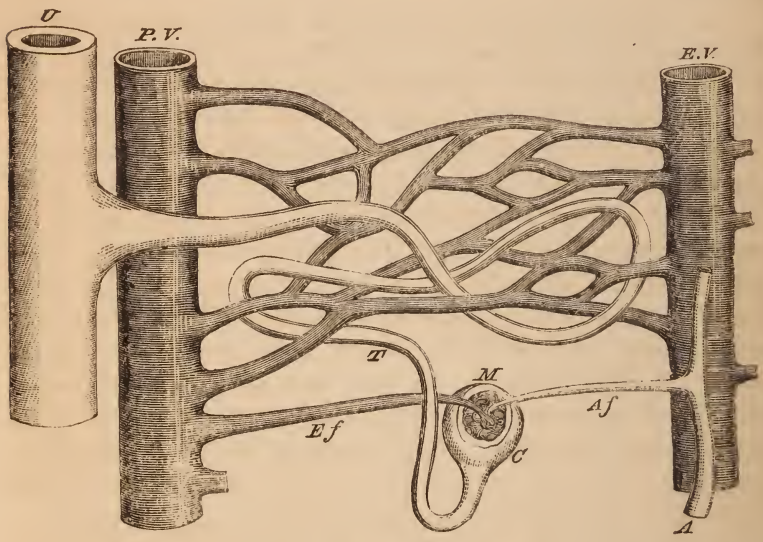

Plate 27.

small twig from it, passing on to expand into and form the Malpighian tuft or coil of capillaries; *Ef* is a vein returning the blood of the Malpighian coil; *M* the coil itself; *P V* the portal vein; *EV* the emulgent or renal vein; *C* the capsule of

the Malpighian coil; *U* the ureter; *T* the uriniferous tube lying in the midst of the minute veins which pass from the portal to the emulgent or renal vein.

In further explanation of his views, Mr. Bowman remarks: "Thus the efferent vessels of the Malpighian bodies are radicles of the portal vein, and through the portal vein empty themselves into the plexus surrounding the uriniferous tubes. The only real difference between this form of kidney and that of the mammalia is that here is a vessel bringing blood that has already passed through the capillaries of distant parts, to be added to that coming from the Malpighian bodies, and to circulate with it through the plexus surrounding the tubes. The efferent vessels of the Malpighian bodies run up the surface in order to throw their blood through the whole extent of the capillary plexus, &c. The emulgent vein of the kidney answers to the hepatic vein of the liver. But in the kidney of the higher animals the portal system has only an internal source, and the artery supplying it is proportionally large."

These views of Mr. Bowman, so generally admitted at the present day, will now be considered, and it is with much diffidence, that I feel obliged to differ in opinion with that very distinguished physiologist. And firstly, as to the vein and plexus, which it is said that the "vena porta sends to the kidney, and which ramifies upon the uriniferous tubes," &c. The current of blood in the portal vein, coming from the lower parts of the animal and passing upwards, must be towards the liver. The direction of the current in the renal vein must be from the kidney towards the vena cava ascendens. A plexus is found to intervene between the vena porta and the emulgent vein. (See Plate 27.) From the blood of this plexus, it is believed that the urine is separated by the action of the epithelial cells which line the uriniferous tubes.

Now, it may be asked, why should this plexus exist, if not for this purpose? To which it might be answered, that by means of this vascular arrangement, a certain amount of portal blood, which is not required for the function of the liver of the serpent, or which might be injurious to its action, can be diverted from that organ by passing through the plexus into

the emulgent vein. This is also probably a channel by which the lower organs of the body may temporarily relieve themselves when congested, by allowing the blood which returns from them into the portal vein to pass into the renal vein, and so on into the current of the general venous circulation. The "vas efferens," or the minute vein which returns the blood of the Malpighian tuft of the serpent, passes into or joins the portal vein. (See Plate 27.) Now, even in the boa, the blood of the "vas efferens" cannot strictly be regarded as portal blood, or at least, it does not as such influence the secretion of urine, unless perhaps when some obstruction occurs in the portal vein. Blood would then pass backwards, or regurgitate from the portal vein into the "vas efferens." But this would be accidental, and would not occur under ordinary circumstances. On the contrary, the blood of the "vas efferens" should be regarded as renal blood. It is, in fact, merely the returning blood of the Malpighian tuft, passing towards and emptying into the portal vein. (See Plate 26.) But the question might be asked, Why then does a portion of the venous blood of the kidney of the serpent return through the vas efferens into the portal vein?

To this it may be answered, that inasmuch as scarcely anything, with the exception of uric acid and urate of ammonia, is separated from the blood of the serpent in the process of urinary secretion, the venous blood of the kidney remains loaded with the elements of urea and of other products resulting from the decomposition of the tissues, and especially from the food of which these animals consume enormous quantities at a time. These elements, then, added to the blood of the vena porta, contribute to it something which is useful in the formation or secretion of the bile. The kidney of the serpent, therefore, by sending or contributing a portion of its venous blood to the liver, stands nearly in the same relation to that organ as do the chylopoietic viscera. Having thus far dissented from the views of Mr. Bowman respecting the peculiar vascular arrangement in the kidney of the serpent, it next remains to be considered whether a similar structure exists, as has been stated, in the human kidney. In the latter we find no communication

between the portal and renal veins, nor any venous plexus resembling that which exists in the kidney of the serpent. Next, as to the analogy which has been drawn between the vas efferens of the kidney of the serpent and the same vessel in the human kidney, or in those of the higher animals. The vas efferens in the serpent returns its blood into the portal vein. The vas efferens of the human kidney returns its blood into a plexus, which may be said to be composed of the minute radicles of the branches of the renal vein. Thus far there is no resemblance between the vascular arrangement of the kidney of the boa and that of the higher animals. But this has been thought to exist between the vas efferens of the human kidney and the vena porta, and it has even been said, that "each efferent vessel is a portal vein in miniature." In order to prove this assertion, it would be necessary to show that the vas efferens originates by very many branches from the Malpighian tuft, in a manner similar to that by which the vena porta commences: viz., by numerous radicles from the abdominal viscera. Mr. Toynbee says: "In the healthy corpus Malpighianum it is impossible to ascertain with precision the manner in which the emerging vessel of the corpus (coil) takes its rise from the rete or coil; but, from an examination of the Malpighian body when morbidly enlarged, it would appear that it springs from the convoluted vessels by radicles of smaller dimensions than the convolutions themselves. In some instances the rete or coil seems to be formed by a single branch of a single vessel in which the artery terminates."

On examination of many injected Malpighian tufts, I have generally seen the vas efferens emerging from the tuft at a point very near to that at which the minute arterial twig, which supports the tuft, enters into it. (See Plates 3 and 9.) I have never been able clearly to perceive the mode in which it originates, and it is very difficult to ascertain this point. I have, however, never seen it commencing by radicles similar to those of the vena porta. But even allowing the arrangement to be exactly as described by Mr. Bowman, the analogy would appear to be a forced one, for the vena porta conveys blood from the intestines to an organ of entirely different structure,

viz., the liver; whereas the vas efferens conducts blood from the Malpighian body of the kidney to a plexus belonging to the *same organ*, and in microscopical proximity to it. Moreover, as it emerges, it is frequently joined by an arterial branch; so that, after all, the so-called venous plexus is not strictly such, but contains a mixture of arterial and venous blood. (See Plate 3.)

It may then be concluded that the interpretation of the peculiar structure in the kidney of the boa-constrictor is not satisfactory, and that no such structure exists in the human kidney, or in that of the larger animals, and that the supposed analogy of the vas efferens of the human kidney with the vena porta should not be admitted, inasmuch as its mode of origin is not clearly ascertained, and at least does not appear to be similar to that of the vena porta; and even if it were as stated, it would not then be proved that *its function* was similar to that of that vessel. The reasons for this view have already been given. It has been attempted to explain the physiology of the human kidney by considerations of structure in the kidney of the boa-constrictor, an animal presenting an immense difference in its organization from that of man, or of the higher animals. The facts and arguments which have here been presented, in opposition to this view of the subject, it is hoped will be found to be satisfactory.

It will be remembered, that to the epithelial cells which line the uriniferous tubes has been attributed the power of separating the urea, lithic acid, &c., from the blood of the venous plexus. How probable this opinion may be will appear from a consideration of the following facts: 1. The Malpighian tuft is covered by nucleated cells. 2. The proximate elements of the urine exist ready formed in the blood. 3. The blood thus containing these elements, as urea, lithic acid, salts, &c., &c., *must* pass into the renal artery, and consequently into the Malpighian tuft, where, owing to the arrangement of this coil of vessels, and also to the small size of the vas efferens, its course must be retarded. The constituents of the urine are thus placed under the most favorable circumstances for escaping from the tuft. 4. Foreign substances introduced into the

blood must necessarily pass into the tuft, and some (as the coloring matter of the bile) have actually been detected in it.

From the foregoing facts it may be inferred that the Malpighian tuft separates many of the proximate elements of the urine (in combination with water) from the blood. It appears to be very difficult, or even impossible, to arrive at any other conclusion. There then remain other elements of the urine, which may perhaps be separated from the blood by the action of the cells of the tubes, as generally supposed. The argument, however, in support of this view, that these cells belong to the spheroidal or "*glandular*" variety of epithelium, and must therefore *necessarily* secrete the proximate elements is untenable, as it has been proved that the epithelium lining the tubes is really of the tessellated kind. On the other hand, this fact is not an objection to the opinion in favor of their secreting agency, because, as has been already mentioned, the bile, which is the most complex of all secretions, is separated by the hepatic cells, which are of very irregular form, and resemble tessellated epithelium. It is not probable that all the elements of the urine should pass through the renal artery and the Malpighian tuft, which appears to be specially constructed for their separation, and to offer every facility to their passage into the capsule of the uriniferous tube, merely that they might reach the venous plexus, where they could then be separated. However, it is not impossible that some one or more constituents of the urine, combined with water, may be separated by the agency of the epithelial cells of the tubes, as generally supposed, and perhaps also any element which may have escaped separation while passing through the Malpighian tuft. Comparative anatomy and chemical pathology will hereafter throw much light upon these obscure points of physiology.

Of the Fibrous Matrix of the Kidney.

Some of the most accurate observers have failed to detect the areolar, or the fibrous tissue, in the proper substance of the kidney. Henle, a most learned and excellent histologist, has even said: "Je n'ai jamais aperçu la moindre trace de tissu cellulaire entre les canalicules urinifères."—Henle, *Anat. Générale*, tome ii. p. 512. The fibrous matrix of the kidney was first described by Mr. Goodsir in 1842, and although this structure is of the greatest physiological and pathological importance, as will be shown hereafter, yet its existence has been, and is even now, doubted or denied by several authors. Professor Lionel Beale, of King's College, London, after describing the matrix, in his excellent work on the microscope, published in 1854, observes: "Of late much discussion has arisen with reference to the presence or absence of a fibrous matrix in the healthy human kidney, and observers are not agreed as to which is really the case."

Dr. George Johnson, in his valuable work on the kidney, remarks, that Rokitansky has fallen into the very grave error of representing the normal fibrous matrix as a product of disease. (Page 321.) And again, he states that Frerichs (a very high authority) doubts even the existence of the normal fibrous matrix. It appeared to me, therefore, that this subject required fresh investigation. I have never satisfactorily succeeded in exhibiting it by following the directions usually given for this purpose. It can, however, be always easily and distinctly shown by the following process: Very thin slices of the kidney are to be made with Valentin's knife, put into a long test-tube about one third full of water, and agitated from time to time for two or three hours.

Prepared in this manner, a thin section exhibits under the microscope a kind of mesh, network, or honeycomb arrangement—the cells of the honeycomb, however, having no bottom. In the natural condition of the kidney, the smaller cells or openings transmitted the tubes; the larger cells or openings were occupied by the Malpighian bodies; which last, together

with the tubes, have now been washed out of the cells, although a few are often seen still remaining in situ. (See Plate 28.)

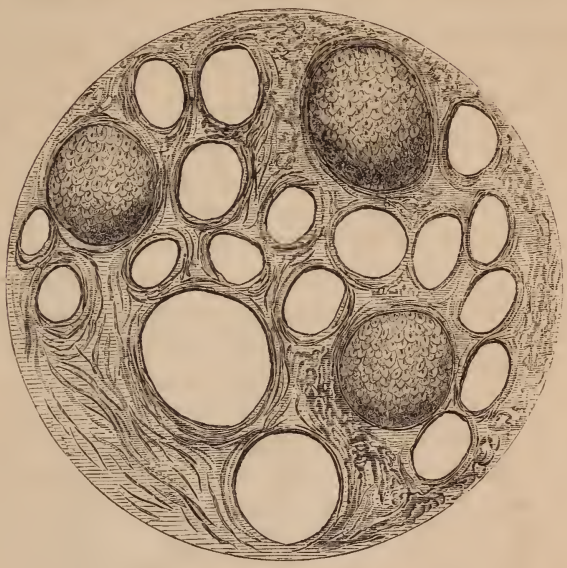

Plate 28.

This plate exhibits the fibrous matrix of the kidney of the rat. The smaller rounded spaces are for the tubes, the larger openings for the Malpighian bodies. Three of the last are also seen. Nucleated epithelial cells are seen through the capsule, and apparently upon the capsule, but really within it. Magnified 250 diameters.

Plate 29 shows the fibrous matrix of the kidney of the dog.

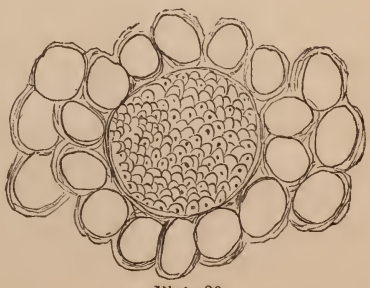

Plate 29.

A Malpighian body is seen in the centre, and surrounded by its own investment of fibrous tissue, and also by numerous ring-like cells for the tubes. Magnified 80 diameters.

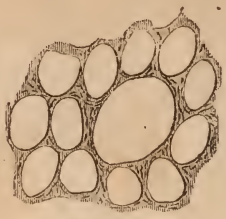

Plate 30.

Plate 30 shows the fibrous matrix of the kidney of the grey rabbit. Magnified 80 diameters.

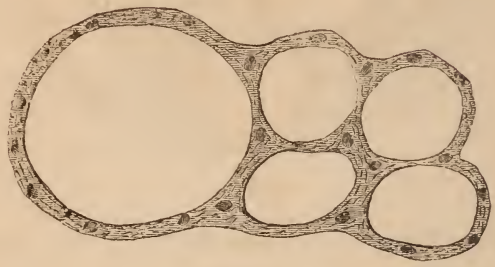

Plate 31.

Plate 31 exhibits the fibrous matrix of the kidney of the

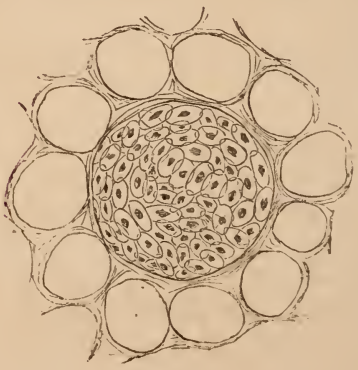

Plate 32.

raccoon. The cut orifices of capillaries can be seen in the meshes of the matrix. Magnified 400 diameters.

Plate 32 shows the fibrous meshes of the kidney of the grey squirrel. The Malphigian bodies and the uriniferous tubes of this animal are very small. The epithelial cells which are on

Plate 33.

the inner surface of the capsule and on the Malpighian tuft, are seen with great distinctness through the transparent capsule. Magnified 400 diameters.

Plate 33 shows the fibous matrix of the kidney of the hog. At *A* is a view of a very thin section of the kidney. Small rounded openings, for the passage of the tubes, and two larger ones for the Malpighian bodies, are here observed. At *B* an artery divides into terminal branches, each of which supports a Malpighian body, enclosed in a ring of the matrix. This is magnified about forty diameters.

Plate 34 shows a Malpighian body lying in its matrix. Nucleated cells can be perceived *apparently* upon its surface. Magnified 250 diameters. This was also taken from the kidney of the hog.

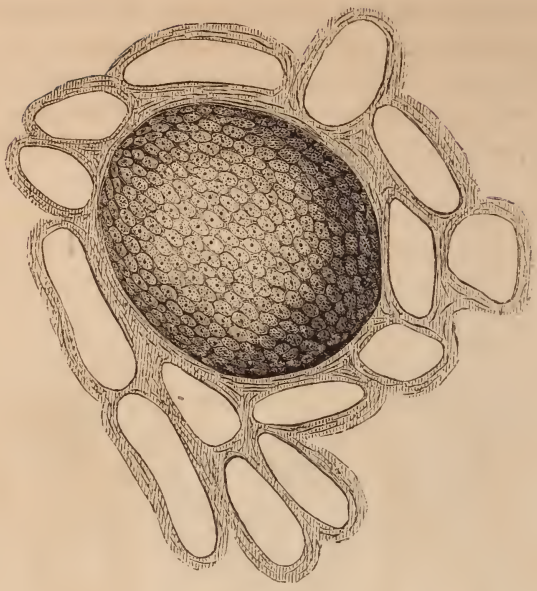

Plate 34.

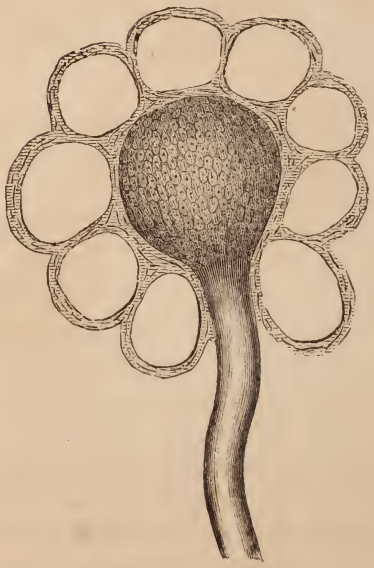

Plate 35.

Plate 35 represents the fibrous matrix of the kidney of the sheep. A Malpighian body can be seen lying in its capsule, which, after surrounding it, passes down to become continuous with the basement membrane of the tube. The epithelial cells lining the tube have been washed away. Magnified 250 diameters.

Plate 36 exhibits a view of the fibrous matrix of the kidney of the ox. The large rounded opening, marked *A*, contained

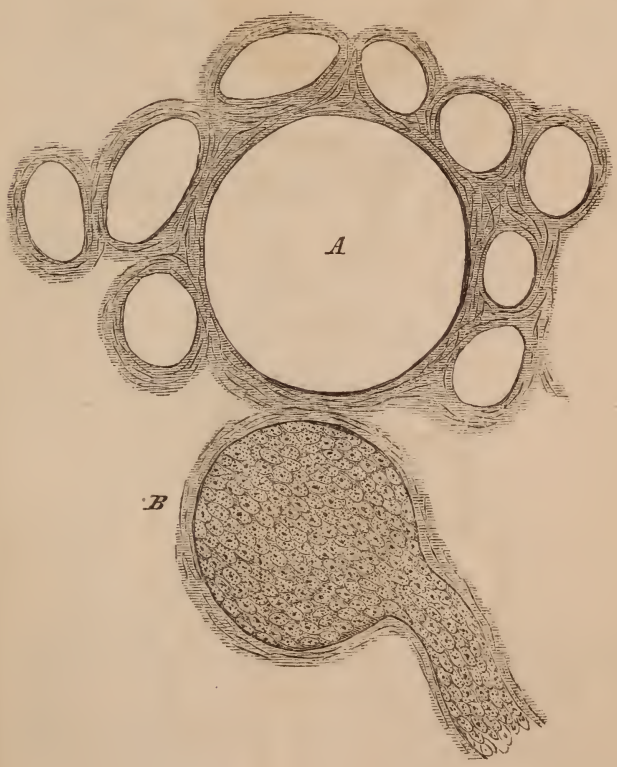

Plate 36.

a Malpighian body. The tubes passed through the smaller rings. At *B* is perceived a Malpighian body. The uriniferous tube is seen to expand and form the capsule, which is enclosed in a ring of the fibrous matrix. Magnified 400 diameters.

Plate 37 shows the fibrous matrix of the kidney of the horse. Magnified 400 diameters.

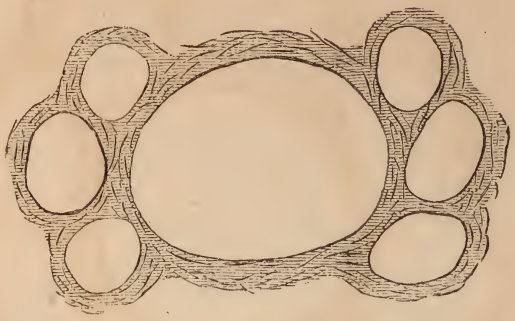

Plate 37.

Plate 38 shows the fibrous matrix of the kidney of the elk. Magnified 400 diameters.

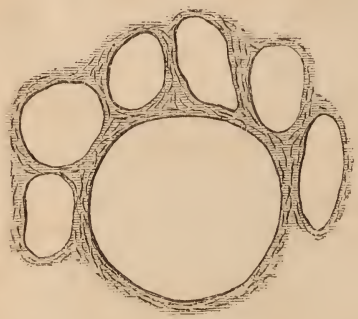

Plate 38.

Plate 39 exhibits the fibrous matrix of the kidney of the moose. Magnified 350 diameters.

Plate 40 shows the fibrous matrix of the kidney of the black bear. Magnified 400 diameters.

Plate 41 exhibits the fibrous matrix of the healthy human kidney. This section was made somewhat obliquely, so that it shows the matrix of the straight tubes which were contained in the elongated spaces, marked *E*. The rounded spaces for the convoluted tubes, and a larger one for the Malpighian body are also seen. Magnified 250 diameters. From a boy, sixteen

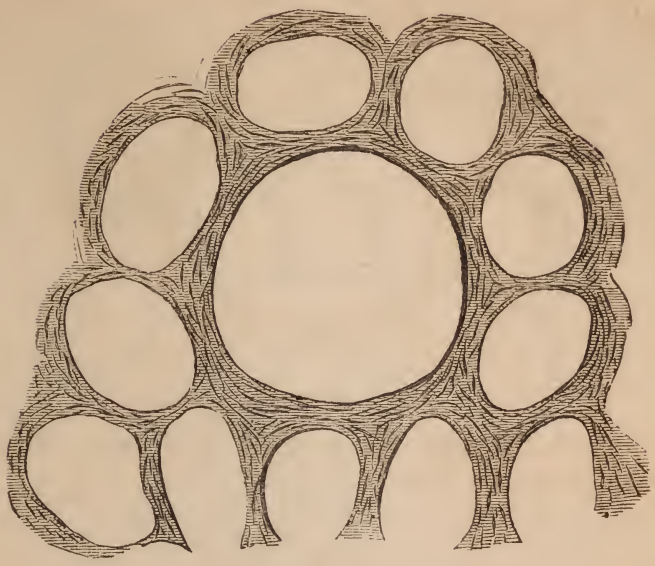

Plate 39.

years of age, killed by falling from a house. This kidney was examined a few hours after death.

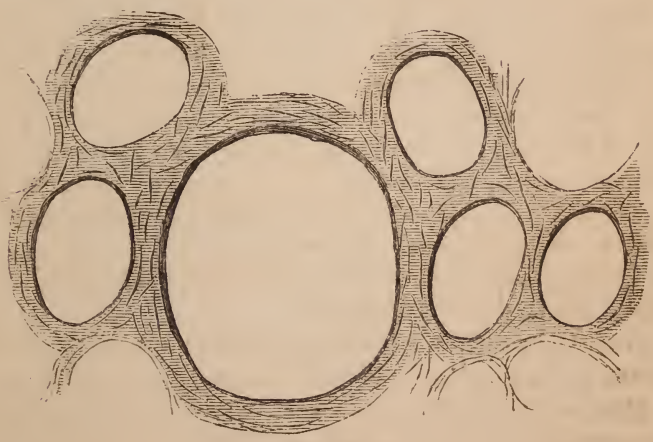

Plate 40.

According to my experience, it is rare to find a human kidney which is *perfectly* healthy. This is particularly the case

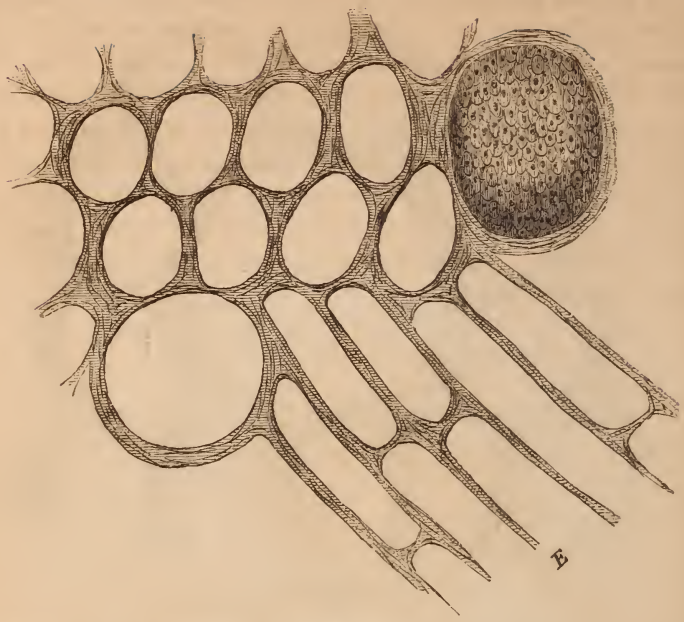

Plate 41.

in subjects in the dissecting-room, and in those who have died in large hospitals. Out of more than 500 subjects which I have examined in this city, I have seen but very few in which the kidney could be regarded as in an entirely healthy condition. Being very anxious to procure a perfectly healthy specimen of the organ, from which I could exhibit the matrix, I obtained a considerable number of kidneys from the bodies of persons killed by violence and accidents, but these were also found to be diseased, most probably from intemperance, &c. I at length procured the healthy organs, from which the present view of the matrix is here given.

From what has now been said and exhibited, it is evident that the fibrous matrix is really the skeleton, or frame-work, of the kidney. It consists of myriads of septa, or partitions, crossing each other in various directions, so as to form elongated spaces for the straight tubes, or rounded spaces, cells, or rings for the Malpighian bodies and convoluted tubes.

In certain pathological conditions of the kidney, it becomes diminished in size and indurated; its surface is irregular and covered with small projecting points, like variously-sized shot. This condition is not unfrequently seen in old drunkards, and would seem to be analogous to cirrhosis of the liver, and probably induced in a similar manner; the alcoholic fluid passing through the tubes of the kidney, and by continued irritation producing crisping, or irregular contraction of the meshes of the matrix, which consequently constrict the tubes and the Malpighian bodies.

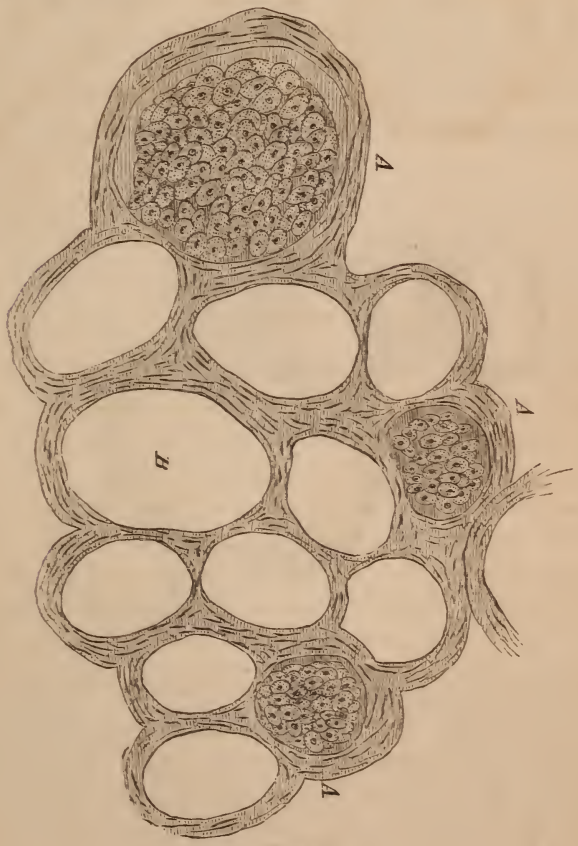

Plate 42.

Plate 42 shows the fibrous matrix of the kidney of a negro.

It was in a condition precisely similar to that just described. The Malpighian bodies were of various size—some of them very small. (See letters *A A A*.) The thickness of the meshes of the matrix was much greater than in the preceding specimen. Magnified 250 diameters.

Thickening and induration of the matrix may produce injurious effects otherwise than by constriction of the tubes and Malpighian bodies. The minute vessels and capillaries pass through the substance of the fibrous rings. Consequently, induration and contraction of the matrix, by direct pressure on the vessels, must greatly interfere with the circulation, nutrition, and secretion of the kidney, and thus various morbid products, as blood, albumen, pus, tubular casts, &c., may be found in the urine.

Plate 43 exhibits the fibrous matrix of the kidney of the sheep (cross section). The kidney had first been injected with

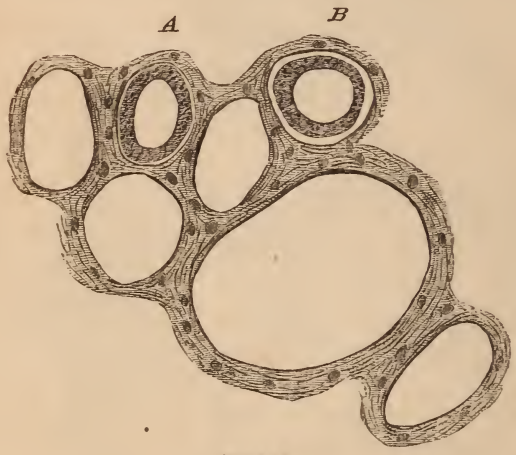

Plate 43.

vermilion through the renal vein, and then thin sections of the organ were carefully washed. The dark, round points, seen in the substance of the rings of the matrix, are the cut orifices of the injected capillaries of the venous plexus. They appear dark when a thin section is viewed under the microscope by transmitted light. Two of the fibrous rings, *A B*, contain a ring-like section of a uriniferous tube, which is still lined by its

epithelium, and its canal may be seen in the centre. Magnified 400 diameters.

To examine into the *nature* of the fibrous matrix, it is necessary to obtain a portion of it unconnected with the tubes, Malpighian bodies, or other parts. After repeated washings and examinations under the microscope, I at length found some thin sections free from any other tissue. On the addition of dilute acetic acid, the matrix became swollen, cloudy, softened, and easily torn or broken, and a number of elongated bodies, or nuclei, appeared scattered through it.

Plate 44 shows the fibrous matrix of the healthy human kidney, treated with acetic acid. Magnified 250 diameters.

Plate 44.

Small portions of the matrix, teased out with needles, in a drop of water, and then examined with the microscope, presented the appearance usually exhibited by white fibrous tissue in other organs, with this exception, that no trace could be found in the matrix of yellow elastic tissue. This last would probably have been injurious, by compressing the Malpighian bodies, and more especially the vessels and uriniferous tubes. It may be presumed, also, that the elasticity of the latter would render the presence of the yellow tissue unnecessary. Hassal, however, says that, "the tubes as well as their globular termina-

tions, are all enclosed in a frame-work constituted of a nucleated form of elastic tissue."—*Micros. Anat.*, vol. i., p. 443. This is very different from what I have observed. While engaged in examining the matrix, I carefully compared it with the white fibrous tissue of tendon, and also with the areolar tissue surrounding the renal artery of the ox just before it enters the hilum, or fissure of the kidney. It is very similar in appearance to both these tissues, and indeed so much resembled the areolar tissue, that no one could have distinguished the difference, with this exception—that in the last could be perceived some fibres of the yellow elastic tissue, while none of these could be seen upon the most careful examination of the matrix. These tissues all exhibited similar reactions with acids and alkalies, and nuclei of the same form and character.

Plate 45 shows the areolar tissue around the renal artery of the ox, acted on by dilute nitric acid. The elongated nuclei

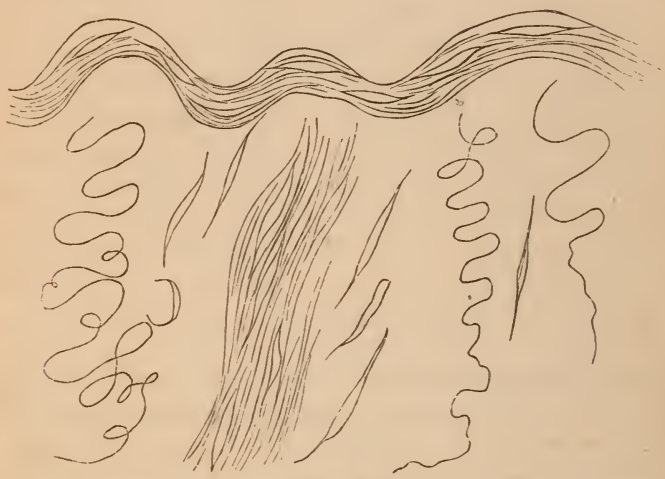

Plate 45.

are seen in the white and waving fibrous tissue. The yellow, elastic fibres are curled like the tendrils of a grape-vine. When the matrix was treated with nitric acid, similar nuclei appeared, but the matrix was not so much softened, or so easily torn or broken. Magnified 250 diameters.

Plate 46 represents the matrix of the healthy human kidney acted on by dilute nitric acid. Magnified 250 diameters.

Plate 46.

The action of muriatic and phosphoric acids was similar to that of the nitric. When portions of the matrix were boiled in water, with one or two drops of sulphuric acid, they shrunk slightly, became soft, easily broken; the rings of the matrix could still be seen, the nuclei very faintly. The prolonged action of all these acids was to render the matrix cloudy and granular, while the nuclei disappeared. They were, however, invariably restored by the addition of the carbonate of potassa.

From what has preceded, we may conclude that the matrix of the kidney is composed entirely of white fibrous tissue, without any admixture of the yellow elastic tissue. I have repeatedly examined it, in many animals and in man, and find it to be in all apparently the same structure, varying only in the size of its meshes or rings, according to the dimensions of the tubes and Malpighian bodies, which differ in all these animals.

From the foregoing remarks, it is evident that a correct knowledge of the fibrous matrix is of great importance, and that its microscopical and chemical investigation, in all cases of diseased kidney, would probably furnish interesting and valuable results.

Something may here be said relative to the proper substance, or parenchyma of the kidney. This consists: 1. Of the uriniferous tubes; 2. Of the arteries, veins, and intermediate capillary plexus—all which have already been described; 3. Of

the lymphatics—of which we know almost nothing, although Hyrtl thinks that they communicate with the Malpighian bodies; 4. Of the nerves—whose mode of termination is unknown, at least in the higher animals. Mr. Toynbee believes that the nervous filaments end by becoming continuous with the parenchyma of the organ, "precisely in the same way as he has observed those in the tail of the tadpole to become directly continuous with the radiating fibres of stellate corpuscles, and the filaments from the corpuscles to communicate with each other." 5. Of the plasma of the blood; 6. Occasionally a few small rounded and granular nuclei can be seen scattered in the substance of the matrix. Lastly, it may be asked, whether any other structure enters into the composition of the parenchyma? Some have supposed that certain peculiar corpuscles, or cells, were also present in this substance; but, after very many and repeated observations, I have not been able to perceive any other cells than those contained in the uriniferous tubes. These cells seem to be united by means of a slightly opaque, amorphous fluid, containing granules, and which is rendered transparent by dilute boiling acids. Many careful observations have not enabled me to discover any other structures entering into the composition of the parenchyma or proper substance of the kidney, than those above stated.

Before concluding this treatise, it may be well to consider what facts or deductions have been brought forward which may be regarded as new, or of an original character, or as tending to confirm or disprove the views of the best authorities on the subject.

1. Certain processes have been recommended for displaying the structure of the kidney, which, so far as I know, have not hitherto been employed. At the same time, views exactly similar (although not always so clear and extensive) have been obtained in the most simple manner, and where no chemical agent has been used, so that the various processes, although entirely different, yet confirm each other.

2. Contrary to the opinions of some of the highest authorities (see G. Johnson, *On Diseases of the Kidney*, p. 35), the epithelium lining the tubes of the kidney has been shown to be of the

pavement or tessellated variety. (See Plates 1 and 2.) The physiological inferences from this anatomical fact have already been considered.

3. Ciliary motion, although weak and imperfect, has been seen in the kidney of the higher animals.

4. I cannot but consider that the views of Mr. Bowman concerning the connection of the Malpighian body with the uriniferous tube are indubitably correct, inasmuch as I have observed this arrangement so very often, and under circumstances entirely different from those under which his observations were made. His plate, however, showing the connection of the expanded extremity of the uriniferous tube (or the capsule) with the Malpighian tuft, together with the "vas efferens" returning the blood of that tuft into the venous plexus, and which has been so generally copied into the text-books on physiology, is merely a diagram, or "plan," and is so called in his paper. By the processes which have here been recommended, I have been enabled to obtain a similar view from the field of the microscope. (See Plates 10 and 12.)

5. The existence of oval, nucleated cells upon the Malpighian tuft of the higher animals has been clearly demonstrated by certain processes which, it is believed, are original. (See Plates 24, 25, and 26.)

6. Some facts and arguments adduced in this paper have led to the conclusion that the function of the Malpighian tuft is to separate from the blood many of the proximate elements of the urine in combination with water.

7. It is not improbable that any elements of the urine which may have escaped separation from the blood, while they were still in the Malpighian tuft, would pass on into the venous plexus, and, in combination with water, be separated from it by the epithelial cells of the uriniferous tubes.

8. The physiological views of Mr. Bowman, which have been so generally received, are not here adopted, and for reasons previously stated.

9. A mode of readily exhibiting the fibrous matrix of the kidney, and views of this structure in different animals have here been given.

10. The arrangement of the minute vessels as they pass through the substance of the matrix has been shown, and also the effect of its constriction upon the vessels in cases of induration of the kidney.

11. The appearance of the matrix in this disease, and its effect upon the circulation, nutrition, and secretion of the organ. I am aware that the views expressed upon this point are not in accordance with those of some excellent pathologists.

12. The chemical and histological nature of the matrix.

13. With regard to the Plates, several views have been given, which may be regarded as new. They were all drawn from nature, with the exception of Plate 27, which was copied from the very excellent paper of Mr. Bowman in the *Philosophical Transactions.* The plates were drawn from the field of the microscope by Henry A. Daniels and Sicard David, anatomical draughtsmen of this city, whose services were the best that I could command. The greatest care was taken to insure accuracy in every instance. The woodcuts were made by Mr. Jacob Wells, 52 John Street, who is well known as one of the best engravers in this city. The microscopes employed were those of Nachet, of Paris, and of Powell and Lealand, of London.

14. In conclusion, it will be remembered, that the investigation of this subject was undertaken with the view of ascertaining, if possible, the true structure of the kidney, and with the hope of thus, perhaps, settling the disputed points relative to its anatomy. Entering upon this examination without any preconceived theory, it has occupied much of my attention for some years past, and for the last twelve months I have labored upon the subject almost daily, sparing neither time, trouble, nor expense, and carefully guarding against arriving at any conclusion until after long continued and repeated examinations. The facts from which deductions have been drawn have all been obtained by the careful examination of the kidney in many animals, and especially in the frog, turtle, snake, alligator, fish, bird, mouse, rat, squirrel, cat, dog, raccoon, rabbit, hog, sheep, deer, elk, moose, ox, horse, black bear, rhinoceros, monkey, and in man. Among the important and disputed questions

which have been considered, are those of the nature of the epithelium in the uriniferous tubes; of ciliary motion in the kidneys of the mammalia; of nucleated cells upon the Malpighian tuft; of the connection of that tuft with the uriniferous tube; of the function of the Malpighian tuft, and of that of the epithelial cells; of the arrangement of the minute vessels of the kidney; of the fibrous matrix—its nature and condition in health and disease. It now only remains for the better judgment of the Academy to decide whether these long-disputed points can now be regarded as satisfactorily determined.

I take this opportunity of expressing my sincere thanks to the Academy for their kind and patient attention.

ON THE FUNCTION

OF THE

MALPIGHIAN BODIES OF THE KIDNEY.

By C. E. Isaacs, M.D.,

DEMONSTRATOR OF ANATOMY IN THE UNIVERSITY OF NEW YORK.

READ FEBRUARY 4, 1857.

In March last I read a paper before the Academy of Medicine upon the Structure and Functions of the Kidney. It may, perhaps, be remembered, that at that time I took occasion to dissent from the opinions of Mr. Bowman (whose views have been so generally adopted by the profession), not only with respect to the nature of the venous plexus, &c., but also in regard to the structure and function of the Malpighian coil or tuft. I then concluded, that instead of merely "separating water from the blood, while the urea, lithic acid, salts, &c., are separated by the epithelial lining of the uriniferous tubes from the blood of the venous plexus, which surrounds them," these tufts, on the contrary, were actively engaged in secreting several of the proximate elements of the urine. This view was deduced from the following facts:

1. That the urea, lithic acid, and salts of the urine have been found to exist in the blood of healthy animals. That in various diseases, and especially after extirpation of the kidneys, these substances are found, in large proportion, in the blood. It is, then, proved that they pre-exist in the blood, and are merely separated from it in the kidney.

2. The renal artery which supplies the kidney *must* neces-

sarily contain these proximate elements of the urine, and as the Malpighian tufts are placed upon the terminal twigs of this artery, they *must* consequently enter the tuft. A glance at the structure will lead inevitably to this conclusion.

3. Most diuretics enter the circulation and can be detected in the urine. They can arrive at the kidney only through the renal artery, and consequently must enter the Malpighian tuft. It would be too tedious and unnecessary to mention the various substances which, after having been absorbed, and having entered the circulation, have been found in the urine. I will only refer to a few which have been given by authorities: salts of iodine, bromine, sulphur, arsenic, antimony, mercury, copper, silver, lead, iron, mineral and organic acids and their salts, as the acetates, citrates, tartrates, &c., &c.; the balsams, cubebs, garlic, turpentine, asparagus, &c.; also quinine and morphine, &c., &c. *Urea, when given internally, acts as a powerful diuretic.*

4. According to Mr. Bowman, the Malpighian coil or tuft lies naked in the capsule of the uriniferous tube; which is a proof that it does not separate the complex proximate elements of the urine from the blood, but merely water, the coil or tuft being, as he says, "a bare or naked system of capillaries." Now I have conclusively demonstrated, by various processes, that the Malpighian tuft or coil is covered by oval, nucleated cells, which are differently affected by chemical reagents from those which line the capsule, and consequently have a different organization. The Malpighian tuft is evidently, then, a glandular structure, every way adapted for the separation of the proximate elements of the urine.

5. In opposition to the view that the Malpighian tuft is merely for the separation of water from the blood, it may be stated, that in serpents, alligators, turtles, &c., the Malpighian bodies abound throughout the cortical substance, and in some of these animals, throughout the whole structure of the kidney, yet their urine is secreted in the semi-solid state. It is not easy to reconcile this view with the theory that the Malpighian tuft merely separates water from the blood.

6. In the kidney of a patient who had been jaundiced for

several years I ascertained the presence of the coloring matter of the bile in the capsule of the Malpighian tuft, and in the commencement of the uriniferous tube. By the application of a drop of nitric acid, the Malpighian bodies exhibited the usual changes of color which are seen when this acid is brought in contact with bile. I therefore concluded, that in this case the coloring matter had been separated by the Malpighian tuft, passed into the capsule, and downwards into the uriniferous tube.

7. Within the last three months, I have performed several experiments upon animals by introducing into the stomach and intestines various coloring matters, capable of being absorbed, entering the circulation, and passing into the kidney. My object was to ascertain whether they would be detected in the uriniferous tubes alone, in which case it might be supposed that they were separated by the epithelial cells of the tubes from the blood of the venous plexus, as believed by Mr. Bowman; or whether they would be found in the Malpighian body as well as in the tubes, when there would seem to be no doubt that the Malpighian tuft separated the coloring matter, which had then passed into the capsule, and so on into the uriniferous tubes. Being very anxious then to ascertain what parts of the minute structure of the kidney would be found to be engaged in this process of separation or secretion, I performed the following experiments. In all of them the animals were put under the influence of sulphuric ether, so as to produce insensibility during the operation:

No. 1. I opened the abdomen of a dog, and tied both ureters; a ligature was then placed on the ileum, near its entrance into the cœcum. A small opening was made into the duodenum, about six inches from the stomach, and two ounces of a mixture of finely pulverized indigo and water were injected into the stomach. The tube of the syringe being again placed in the same opening, four ounces of the mixture were injected downwards into the jejunum and ileum. Ligatures were placed on the intestine, above and below the opening, in order to prevent the escape of the injected fluid. The abdomen was then closed by the glover's suture. This animal lived twenty-eight

hours after the operation, and died with the usual symptoms of poisoning, by retention of the urea. On *post-mortem*, there was very little peritonitis. On making thin sections of the kidney, the Malpighian bodies and uriniferous tubes were found very much darkened by the indigo. Both the bodies and the tubes were of various shades of dark blue and black, some being much darker than others.

No. 2. I operated upon a cat in a similar manner, using however a solution of deoxidized indigo, prepared according to the process of the dyers, with sulphate of iron and lime-water. By this process the indigo becomes very soluble, and of a light yellow color, absorbing oxygen, and turning blue on exposure to the air. On dissection, not a trace of indigo could be found in the kidney. I do not know to what cause the failure of this experiment should be attributed.

No. 3. I repeated the operation upon a small white dog of feeble constitution. This animal only lived fourteen hours. No indigo could be detected in the kidney. I think the cause of failure in this case was owing to the fact that the solution was made with cold, instead of warm water, which should always be used, and also to too long exposure of the intestines to the air.

No. 4. I injected the mixture of indigo and water into the stomach and intestines of a very large and ferocious black cat. The ureters were tied. He lived thirty-four hours. There was scarcely any peritonitis. Both the Malpighian bodies and uriniferous tubes were of a light blue, and some of a very dark color.

No. 5. I repeated the same operation on a kitten, which lived fifteen hours. The Malpighian bodies and tubes were of very dark color, as in the preceding experiment.

No. 6. In a large grey cat I removed the left kidney, and tied the right ureter, hoping that the remaining kidney, by receiving more indigo, would show the Malpighian bodies and the tubes more deeply colored. This cat lived twenty hours. Scarcely any peritonitis. The Malpighian bodies and uriniferous tubes were much darker than those of the animals of the preceding experiment. Some of them were nearly as black as

ink. It is from a thin section of the kidney of this cat, under the microscope, that the drawing and diagram have been made by my friend Mr. Benj. Howard, an excellent artist, and a student of medicine in the University of this city.

Plate 47.

No. 7. On the 28th of January I injected six ounces of a mixture of finely pulverized indigo and water into the œsophagus of a small dog. Eighteen hours after, I again injected six ounces more. Twenty-four hours afterwards, I injected six ounces of the mixture. Twenty-one hours after, four ounces were injected; and three hours after this last injection, the animal was killed. I may here mention that a brass pipe with a stop-cock had been left in the œsophagus, and secured in it by a ligature, so that the successive injections were made with great ease and convenience. On examination, only a small number of the tubes and Malpighian bodies were colored. Some of the last were of a light green color, others were slightly darkened; a few were more deeply colored. The tubes were generally uncolored, except here and there in the pyramidal portion of the kidney, where some of the straight tubes were nearly black. I regret that I could not inject the indigo in this animal at such times as I could have wished; could I have done so, the result would probably have been more satisfactory. It will thus be seen that out of seven experiments with indigo, five succeeded, the Malpighian bodies and uriniferous tubes being both colored by it. Berzelius found in indigo a brown, red, and blue pigment. This fact may explain the shades of color which were seen in the Malpighian bodies and uriniferous tubes. These organs may perhaps also produce some slight change in the coloring matter in its passage through them. In some thin sections of the kidney, the Mal-

pighian tuft and capsule seemed to be filled with colored material, which could be traced along the tube, gradually becoming lighter in its progress downwards. In some of these animals the urine in the ureters, above the ligatures, was of a slightly blue color.

No. 8. On the same day I injected a solution of the ferrocyanide of potassium into the stomach and duodenum of a large cat, and then placed a ligature above the opening which had been made into the intestine in order to inject the solution. About ten inches below this point I made another opening into the intestine, and injected downwards into the jejunum and ileum a weak solution of sulphate of iron. A ligature was placed just below this opening, so that the contents of the intestine could not escape. The two solutions were thus separated from each other by ten inches of intestine which contained none of the solution. Ligatures had previously been placed on the ureters. This experiment was performed at 3 P.M. on Saturday, and the animal was found dead on Monday morning, so that it is not known how long she lived after the operation. The Malpighian bodies and uriniferous tubes were of a dark blue color, showing that Prussian blue had been formed in the course of the circulation, and had passed through the Malpighian bodies and uriniferous tubes. The urine was of a slightly blue color.

No. 9. I may also state that I have injected a mixture of Prussian blue and water into the stomach and intestines of a cat, and have found the Malpighian bodies and tubes of a dark blue color; but I regret to say that I have lost the notes of the experiment, although the specimen of the kidney is still in my possession.

No. 10. December 24, at 4 P.M., I tied the ureters of a small dog and injected a solution of madder into the stomach and intestines. This dog was found dead on the 26th. The Malpighian bodies were of a brilliant red color, exhibiting a most beautiful appearance. The tubes were dark red, and some of them very darkly colored. Thin sections of the kidney were placed under the microscope, and then touched with a drop of solution of caustic soda or ammonia, when the Mal-

pighian bodies and tubes became of a still more brilliant red color. The urine which was retained in the ureters, above the ligature, was acted on similarly by the same chemical reagents.

No. 11. On the 22d of January, at 2 P.M., I injected a solution of madder, containing a small quantity of bi-carb. soda, into the stomach and intestines of a dog. This animal died at 8½ P.M. the same day. The ureters had been tied. The Malpighian bodies were of a bright red color. Some of the tubes were simply darkened, others of a dark red color. The fluid in the ureters, above the ligatures, was reddened on the application of carbonate of soda, and also of aqua ammoniæ.

No. 12. I injected a solution of the Brazil wood, *Cæsalpinia Crista*, into the stomach and intestines of a dog, having previously tied the ureters. On examination, sixteen hours after the operation, the Malpighian bodies were of a bright red color; the tubes were darkened. The red coloring matter of Brazil wood is very sensible to the action of alkalies, and on touching thin sections of the kidney, under the microscope, with solution of ammonia or soda, the Malpighian bodies became of a purple color. The tubes exhibited this change only in a slight degree, and many of them not at all.

No. 13. On the 24th of December last, at 12 M., I tied the ureters of a large dog, and injected a solution of cochineal into the stomach and intestines. He was seen alive at 5 P.M., and found dead on the morning of the 26th. The Malpighian bodies were very red; some of the tubes dark red, and a few of them were nearly black.

No. 14. On the same day, at 3 P.M., I tied the ureters in a cat, and injected a mixture of carmine and water into the stomach and intestines. She was seen alive at five P.M., found dead on the 26th. The appearance of the Malpighian bodies and the tubes was the same as in the preceding experiment.

No. 15. I injected into the œsophagus of a dog twelve ounces of a mixture made by rubbing in a mortar two drachms of powdered cochineal with half a drachm of bi-carbonate of soda, to which one ounce of glycerine was added, and afterwards eleven ounces of water. At the end of three hours the animal

was killed. The appearance of thin sections of the kidney, under the microscope, was exceedingly beautiful. The Malpighian bodies were distended, and of a purple and purplish red color. Some of the tubes were light red, others were uncolored. On adding a drop of solution of ammonia, the color of all these parts was changed to a deep red.

No. 16. I injected into the œsophagus of a cat three ounces of a similar mixture of cochineal and three ounces of infusion of madder. At the end of three hours this cat was also killed. The Malpighian bodies and the tubes were of similar, but not as deep color as those of the dog in the last experiment. The same effect was produced on thin sections of the kidney by using ammonia as in the last experiment.

No. 17. January 20th, at 3 P.M., I tied the ureters of a cat, and injected a solution of logwood into the stomach and intestines, having previously fixed a ligature on the upper part of the rectum. This cat was seen living at 9 P.M., and was found dead the next morning. There was no peritonitis, no sign of inflammation or congestion, except at one point, just below the ligature, on the rectum. Thin sections of the kidney showed the Malpighian bodies of a very red color. Under the microscope both these bodies and the tubes were darkened by the solution of the sulphate of iron. The urine retained in the ureters and in the pelvis of the kidney was also slightly darkened by the same reagent.

No. 18. On the 30th of January, at 1½ P.M., I placed a ligature on the duodenum of a cat, and another about the middle of the jejunum. Into this portion of included intestine I threw a solution of logwood. An opening was then made below the lowermost ligature, and a weak solution of sulphate of iron was injected, a ligature having been placed on the ileum just above the ileo-cœcal valve. This cat died at 8 P.M. the same day. The Malpighian bodies were dark red, and much distended; the tubes of a very slightly blue color. Neither were affected by dropping upon them the solution of sulphate of iron. This experiment was unsatisfactory, and the cause of failure is perhaps to be attributed to the fact that too large a quantity of the solution was injected, and the intestines consequently be-

came over distended. Absorption, therefore, did not fully take place.

No. 19. On the 2d February, at 6 P.M., I injected six ounces of a mixture of finely pulverized turmeric and water into the œsophagus of a cat. I repeated this injection several times, and on the 4th, at 4 P.M., she was killed. I could not detect the slightest trace of this coloring matter either in the kidney or the urine, nor even by chemical tests.

No. 20. On the 4th day of February I injected several times into the œsophagus of a cat an infusion of logwood, madder, and Brazil-wood. Four hours after she was killed. The Malpighian bodies were of a faint purplish red. On applying ammonia to thin sections, under the microscope, the color was increased. The bladder was full of a pink-colored urine, which was strongly reddened by ammonia, and also by caustic soda.

No. 21. I procured some of the Persian berries, or berries of Avignon, which are much used by the dyers in coloring cloths of a yellow color. In experimenting with this substance, I found that it contained a yellow coloring matter similar to that of the bile, inasmuch as when acted on by nitric acid it showed a play of colors similar to that which is exhibited when bile is treated with the same reagent. I also ascertained that the yellow coloring matter of Spanish saffron showed a transient green color when acted on by nitric acid. I injected an infusion of equal parts of Spanish saffron and the Persian berries into the œsophagus of a dog, to the amount of eight ounces, and repeated this injection every hour for four hours, at the end of which time the dog was killed. The Malpighian bodies and uriniferous tubes were of a yellow color. On adding a minute quantity of dilute nitric acid to thin slices of the kidney, under the microscope, a play of colors was seen similar to that which was observed when some of the mixed infusion of the Persian berry and saffron was poured on a white plate, then allowed to run off by inverting it, and finally a drop of nitric acid suffered to fall upon the plate, when the rapidly changing colors were instantly perceived.

If we now review the result of the experiments up to this point, it will be seen that of the various coloring matters which

have been detected, both in the Malpighian bodies and in the uriniferous tubes, by means of the microscope and by chemical tests, indigo has been found five times, madder twice, cochineal twice, and Prussian blue also twice. Carmine, the mixture of Persian berry and saffron, logwood and Brazil, have each only once been tried, and have all been detected in the kidney, as has also been the mixture of logwood, Brazil-wood, and madder, and likewise a mixture of madder and cochineal.

I may here state, that in none of my experiments with coloring matters have I ever detected them in the venous plexus of the kidney.

No. 22. On the 20th of December last I injected the stomach and intestines of a cat with a mixture of animal charcoal and water, having previously ligated the ureters. The animal lived twenty hours. Both the Malpighian bodies and the uriniferous tubes were of a very dark color, many of them being intensely black.

No 23. At 4½ P.M., on the 2d of February, I injected six ounces of a mixture of lampblack and water through the œsophagus of a cat. At 10 P.M. I repeated the process; also at 1 P.M. next day, and again at 3 P.M. The animal was killed two hours after. The Malpighian bodies and uriniferous tubes were of various shades of blackness, some being intensely black.

No. 24. At about 4 P.M. on the 2d of February, I injected the œsophagus of a cat with a mixture of animal charcoal and water, to the amount of about eight ounces. By mistake the animal was killed about an hour afterwards. The uriniferous tubes throughout the kidney were very dark, some of them intensely black. Many of the Malpighian bodies were darkened, but generally were not so much so as the tubes.

Nos. 25 and 26. On the 3d of February, I operated upon two cats, injecting the œsophagus of both animals with a mixture of animal charcoal and water, repeating the injection several times at intervals until they were killed, which was about three hours after the operation. In both of these cats the Malpighian bodies and uriniferous tubes were of various shades of blackness, some of them being intensely black.

I should have previously remarked, that in some Malpighian bodies the various coloring substances were sometimes seen in the centre of the capsule, while a clear interval existed between the colored portion and the inner surface of the capsule; thus showing that the coloring matter had not yet left the tuft. In other instances the central portion corresponding to the tuft was only slightly colored, while a deeply colored part was situated between the tuft and the internal surface of the capsule.

I think these experiments conclusively show that the Malpighian tuft can and does separate coloring matters from the blood which have been absorbed from the stomach and intestines, and have thus entered the circulation.

No. 27. On the 19th of January I tied the *ductus communis choledochus*, or common bile-duct, of a large grey cat, with the view of allowing the elements of the bile to remain in the circulation, and the absorption of any which might have been already secreted. I hoped thus to be able to detect it either in the tubes or the Malpighian bodies of the kidney. This cat lived seventy-five hours, and seemed to suffer but little from the operation. On taking off the skin, I observed the under surface of it, in several points, to be of a bright yellow color. The bile-duct was partially ulcerated, so as to allow of the escape of a few drops of bile. There was very slight peritonitis. Thin sections of the kidney exhibited the Malpighian bodies of a light yellow color. Nothing remarkable was observed in the tubes. On adding a portion of a drop of nitric acid to the specimen under the microscope, the Malpighian bodies showed a slight play of colors, among which blue, green, and violet could be most distinctly seen. The same effect was seen in the tubes, but not so clearly. It was observed also on testing the urine. There could then be little doubt of the presence of bile in the Malpighian body and uriniferous tubes.

No. 28. I repeated this experiment on a large yellow dog, and in addition tied the ureters. This animal lived forty-eight hours. The Malpighian bodies were somewhat congested, and of a yellowish red color. I was much disappointed at the result of this experiment, as I could not detect the presence of bile in any part of the kidney, or in the urine, although I used most

carefully and repeatedly all the known tests for bile. I then made comparative tests with the bile from the gall-bladder of this dog, and what was exceedingly surprising, I could not succeed in producing the usual peculiar appearances, either with nitric acid, sulphuric acid and sugar, or any of the usual tests for bile, even after most careful and repeated trials. I know not to what this should be attributed.

I had long been anxious to obtain the kidneys of persons who had been in the habit of passing minute calculi of lithic acid, or whose urine constantly or usually deposited crystals of lithic acid or the lithate of ammonia. It seemed not unreasonable to suppose, that by repeated examinations of such kidneys under the microscope, this acid, or perhaps even its combinations, might there be detected. If found in the tubes alone, its presence could be explained by the theory of Mr. Bowman, viz., that it was separated by the epithelial cells of the tubes from the blood of the venous plexus which surrounds them. On the other hand, if found in the Malpighian tuft and capsule, then we must believe that it was separated by that body. My friend, Dr. F. U. Johnston, jun., has kindly presented me, within a few days past, the kidneys taken from a child three months old. These contained several very small calculi of lithic acid, which were situated in the calyces of the kidney. On making thin sections of the organ, the Malpighian bodies were of a yellow color. Some portions of the tuft were of much deeper color than others, and seemed to contain reddish yellow masses. I endeavored, by using different powers, to ascertain whether these deeper-colored parts were crystalline, or had any determinate form, but could not succeed, after repeated attempts, in resolving this question. The tubes were not remarkable. I added a minute portion of dilute nitric acid to thin sections, under the microscope, and gently heated the specimen over a spirit-lamp, and then exposed it to the vapor of ammonia; then gently heating it again, placed it under the microscope. The Malpighian bodies had become of a deep red, and some of them of a beautiful purple color. I tried these tests repeatedly with the same result. Generally, the tubes were not at first affected; but on further application, many of

them assumed a red or purplish red color. I made comparative tests with sections taken from the kidney of a cat, and also from that of a sheep, testing them in the same manner. In these trials, however, the Malpighian bodies were either rendered of a dirty brown color, or lost their color, being entirely blanched. I made somewhat thinner sections of the child's kidney, and on gently heating them with water and a little nitric acid, effervescence took place, exactly as when lithic acid is heated gently with nitric acid. On then exposing the specimen to the vapor of ammonia, or on adding a few drops of it, and applying heat, a purple or reddish purple was produced. Shall we infer that this was purpurate of ammonia, and if so, that as the lithic acid was thus found in the Malpighian body, it has the power, and does really separate it from the blood? I shall content myself at present by simply stating the facts, and also my conviction that further researches in this direction will probably settle the question.

I removed the kidneys from a subject who had died of aneurism of the aorta, and whose urine had deposited the lithate of ammonia to a very great amount for a few days previous to his death. In examining one of these kidneys, I found the Malpighian bodies greatly congested, and of a dark red color. I then injected the renal artery of the other kidney with two ounces of water to half a drachm of nitric acid. This kidney was then laid aside for twenty-four hours. I examined thin sections of it under the microscope, but could not discover any deposit in the tubes or in the Malpighian bodies. I placed a drop of diluted nitric acid upon a thin section, heated it very gently over a spirit-lamp, and then exposed it to the vapor of ammonia. On placing it under the microscope, the tubes and Malpighian bodies were both of a reddish and reddish purple color. I made comparative tests with thin sections from the fresh kidney of a cat, but in this case the color was entirely destroyed by the application of the above reagents.

I made some experiments to ascertain the effect of highly stimulating diuretics upon the secreting structures of the kidney.

No. 29. On the 24th of January I injected half an ounce

of nitrate of potassa, dissolved in three ounces of water, into the small intestines of a cat. Ligatures were applied as in the preceding experiments. This animal lived thirty-six hours. The mucous membrane of the intestines was very much inflamed. The Malpighian bodies were very red and much congested; many of them were ruptured. The intertubular plexus was greatly congested. The mucous membrane of the bladder was pale.

No. 30. On the 21st of January I injected into the œsophagus of a cat half an ounce of tincture of cantharides, mixed with three ounces of water. This animal lived eighteen hours. There had been no symptoms of excitement of the genito-urinary organs. The Malpighian bodies were intensely reddened, and some of them were ruptured. The venous plexus was very much congested. There was no inflammation of the stomach or intestines; on the contrary, their mucous coat was pale, but that of the bladder was inflamed.

No. 31. January 2nd I injected six grains of phosphorus in three ounces of olive oil, into the duodenum of a cat, a ligature having been placed above the opening into the intestine, and another on the ileum, just above the ileo-cœcal valve. After an hour, there occurred great frequency of respiration and circulation, paroxysmal efforts to bite at objects, and catching and sparring (as it were) with the paws, in the most ferocious manner. Gradually, great prostration and insensibility came on, with dilatation of the pupils. She died three hours and a half after the operation. The mucous membrane of the stomach and intestines was very pale. The Malpighian bodies were greatly congested, and some of them were ruptured. The intertubular plexus was much congested. All these parts were of a most brilliant red color, so that one of my friends remarked that "the specimen of the kidney, under the microscope, seemed to be in a blaze of fire." The kidney had a strong odor of phosphorus.

No. 32. On the 30th of January I injected a grain and a half of phosphorus, dissolved in an ounce and a half of olive oil, into the small intestines of a cat. This animal lived fifteen hours. The Malpighian bodies and venous plexus were very

red and much congested, but not so much as in the subject of the preceding experiment. The bladder was slightly congested. In neither of these animals in which phosphorus was injected was there any symptom of excitement of the genital organs.

No. 33. On the 2d of February, at 6½ P.M., I injected an emulsion of one ounce of spirits of turpentine, beaten up with an egg, and three ounces of warm water, into the œsophagus of a cat. Four hours after, she seemed to be greatly prostrated, and was found dead the next morning. There was no inflammation of the stomach or intestines; the bladder was pale, and contained urine, which had the odor of turpentine. The kidneys were greatly congested; the Malpighian bodies filled with blood, and of a violet red, or rather a purple color. Many of them, as well as the small arterial twigs upon which they are placed, were ruptured. There was also much congestion of the venous plexus.

The foregoing experiments then demonstrate the influence of powerfully stimulating diuretics on the Malpighian bodies and the venous plexus.

Let us now review the facts and arguments which thus far throw light upon the function of the Malpighian tuft:

1. That it is not for the purpose of merely separating water from the blood is proved by its distribution throughout the substance of the kidney in some animals whose urine is semi-solid.

2. The proximate elements of the urine exist ready formed in the blood.

3. These elements can only reach the kidney through the renal artery, upon whose minute terminal branches are placed the Malpighian bodies.

4. It has been shown that the Malpighian tuft is covered by peculiar oval nucleated cells. This glandular structure seems to be, in every respect, well fitted for the separation of these substances.

5. The action of diuretics upon the Malpighian tuft.

6. The actual separation by it of coloring substances, as demonstrated by these experiments.

7. The existence of the coloring matter of the bile in the Malpighian tuft and capsule.

8. The facts here adduced relative to the presence of lithic acid in the Malpighian tuft, and its detection by chemical tests.

9. *From all these considerations, it seems that we are justified in concluding that the Malpighian tuft separates from the blood most of the proximate elements of the urine.*

10. *Any element of the urine which is not secreted by the Malpighian tuft is then probably separated by the epithelial lining of the tubes, as is generally believed. This may also occur with certain foreign substances, which have been introduced into the circulation, and which the Malpighian tuft refuses to separate. It is possible also that certain constituents of the urine may be separated from the blood both by the tubes and the Malpighian bodies. All the facts, however, adduced in this paper, lead to the conclusion that it is to the Malpighian body that we are to attribute the principal agency in the secretion of the urine.*

I may here mention some interesting facts which were observed while making these experiments. The ferrocyanide of iron or Prussian blue, and indigo, are both insoluble substances, and are only held in suspension when mixed with water. Inasmuch, however, as they were absorbed and taken into the circulation, we must either suppose that they were rendered soluble by some substance met with in the intestinal canal; or else, contrary to the general opinion, that it is not necessary for a substance to be soluble in order to be absorbed, pass into the circulation, and then be separated by the kidney. As I had ascertained that animal charcoal, an exceedingly insoluble substance, could be found not only in the kidney, but also in the hepatic cells, and in the minute vessels of the spleen, I determined to try some experiments with other substances well known to be insoluble.

No 34. On the 26th December I removed the left kidney of a cat, and placed a ligature on the right ureter. I then

injected a mixture of very finely pulverized metallic bismuth into the stomach and intestines, using ligatures as usual. The cat died sixteen hours after the operation. The Malpighian bodies and tubes were of a light grey and of a steel grey color, resembling the impalpable powder of metallic bismuth. No chemical tests were employed to ascertain its presence in the kidney.

No. 35. I caused a book of gold-leaf to be very finely ground with sulphate of potassa. This salt was then washed away by water; the finely-powdered gold was then diffused through water by agitation in a vial, and then poured into the small intestine by means of a funnel, passed through the small opening in the duodenum. At the same time the left kidney was removed, and a ligature placed on the right ureter. The dog lived twenty-four hours. There was considerable peritonitis, with effusion of pus and coagulable lymph. There was also some inflammation of the mucous membrane of small intestine, and hemorrhage into the ileum. The Malpighian bodies were of a yellow color, the tube somewhat darkened. I could not satisfy myself that gold existed in the kidney by examination with the microscope. I also tried the method by incineration, both with the kidney and the liver, but could not detect gold in either organ.

No. 36. I ground up a book of gold-leaf with powdered glass into an impalpable powder. This powder was washed repeatedly with water to separate the siliceous portion, and the gold was mixed up with a solution of gum-arabic and injected into the œsophagus of a dog. The animal was killed about six hours after injection, but I could not detect gold in the kidney.

No. 37. On the 5th of January I removed the left kidney of a cat and ligated the right ureter. I then injected finely precipitated copper, in an impalpable powder, into the stomach and intestines, using ligatures in the usual manner. This cat died in three hours. The intestines were slightly, the stomach intensely inflamed. I could not detect the copper in the kidney, either by the microscope, or by the method of incineration in a porcelain crucible.

No. 38. On the 30th of January I injected three drachms

of finely precipitated copper into the œsophagus of a dog, and secured a brass pipe in it with a stopcock. Twenty hours after I injected the same quantity, suspended as before in water. He was killed two hours afterwards. I incinerated one of the kidneys, in a porcelain capsule, with nitric acid; but, testing, could not detect a trace of copper. Neither was I satisfied from the appearances under the microscope that this metal was present in the kidney.

No. 39. On the 2d February, I injected into the œsophagus of a cat three drachms of metallic antimony, ground down to an impalpable powder and well agitated with water at the moment previously to throwing in the injection. The animal was killed five hours afterwards. Many of the tubes were of a steel-grey color. My young friend, Mr. Henry Draper, kindly tested one of the kidneys for me in the laboratory of the University, but could not detect a trace of antimony in the kidney. I therefore consider this experiment as a failure, and also that all the other experiments with metallic substances were unsatisfactory, and that the fact of their absorption and entrance into the kidney has thus far not been demonstrated in these instances.

I may here mention that the introduction of coloring matters into the kidney of animals by means of absorption from the intestines and entrance into the circulation, is a valuable means of facilitating the investigation of the structure of these organs. The effect of stimulating diuretics is also of great assistance in this respect. It seemed to me also that by ligaturing the abdominal aorta a short distance above its bifurcation, a larger amount of blood would thus be thrown upon the kidney, with the effect of congesting the Malpighian bodies and venous plexus. Being unable, from sickness and other causes, to carry out my design, I requested my friend Dr. J. S. Gouley, whose anatomical zeal and acquirements are deserving of the highest praise, to perform this operation, the results of which we examined together. I beg leave here to express my sincere thanks to him for his skillful and valuable assistance in nearly all of my experiments.

Nos. 40, 41, 43. I condense from Dr. Gouley's notes of

three operations upon cats and dogs, in which he ligated the aorta, twice reaching the artery by an incision through the abdomen and once through the lumbar region, without wounding the peritonæum. The symptoms, after tying the aorta, in these three cases, were great acceleration of the heart's action —pulsations so frequent that they could not be counted, no pulsation in the femoral arteries, paralysis and coldness of the inferior extremities, gradually increasing coma and diminished frequency of respiration. On dissection, the heart was distended with coagulated blood; lungs, slightly congested; liver, nearly normal; kidneys, very highly congested. Thin slices of both organs showed, under the microscope, the Malpighian bodies enormously distended with blood; so much so, that on looking at a very thin section with the naked eye, they could be plainly distinguished. Under the microscope, the tortuous vessels of which the tufts are composed could be perceived more beautifully than in an artificial injection. Small vessels could be seen sending branches on each side, terminating in the well known round ball, all filled to their utmost. The venous plexus was also very highly congested. The intertubular vessels were very distinct, being intensely congested. Three or four of these specimens have been preserved.

I may here remark, that the injected state of the vessels resulting from this operation greatly facilitates the investigation of the structure of the kidney. In connection with this congested state of the Malpighian tufts and venous plexus, it may not be uninteresting to refer to the operation of Sir Astley Cooper for ligature of the abdominal aorta, in which case there was suppression of urine for more than twenty-four hours.

In conclusion, it may be well to consider what new facts or deductions have been stated in this paper:

1. It has, I think, been conclusively established, by numerous experiments on animals, by repeated and careful observations under the microscope, and by chemical tests, that colored substances are separated from the blood by the Malpighian body.

2. That the coloring matter of the bile is also separated by it from the blood.

3. From the facts recorded in this paper, it is very probable that it may also separate lithic acid.

4. That a new means of unravelling the structure of the kidney is afforded by such experiments.

5. The peculiar action of excessively stimulating diuretics has been demonstrated by their effect on the minute structures of the kidney.

6. The influence of ligature of the abdominal aorta in throwing an increased afflux of blood upon the kidney, and causing great congestion of its minute vessels and capillaries.

7. Incidentally, some interesting facts have been stated relative to the absorption of substances generally considered to be insoluble.

ADDENDA.

My friend, Dr. Geo. T. Elliot, examined with me this day, March 24th, 1857, the kidneys of a patient who had been under treatment for paralysis from lead for the last seven months in Bellevue Hospital, and who died of tetanus. I made a drawing of the epithelial cells, both of the straight and convoluted tubes, which, under the microscope, exhibited with the greatest clearness and distinctness the same appearances as seen in Plates 1 and 2 of this paper.

I received from my friend, Dr. Geo. A. Peters, the kidneys of a child three months old, who died with convulsions, the urine having been suppressed for fifteen hours. The Malpighian bodies were very much congested, and of a dark red and purple color. The intertubular plexus was also much congested. I have also noticed that the kidneys of other patients who had died from suppression of urine were in a similar condition. This, taken in connection with the influence of stimulating diuretics upon the internal structures of the kidneys of the inferior animals, and the effect of ligature of the abdominal aorta in producing a similar result, would seem to show the importance of local depletion and counter-irritation in cases of suppression of urine and retention of urea in the blood, and the necessity of avoiding diuretics, or at least of using them with great caution.

The successful result of this mode of treatment in such cases is in accordance with the views expressed in this paper relative to the physiology and pathology of the Malpighian bodies and venous plexus.

END OF THE FIRST VOLUME.

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